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THE UNIVERSITY IN MODERN SOCIETY

SIR WALTER MOBERLY has had a life-long experience of British universities and their problems, derived as a tutor at Oxford, as vice-chancellor of a great civic university, and since 1935 as chairman of the University Grants Committee. Among these problems he moves with the ease of long intimacy and deep understanding, and in his recent book* he writes with a charm and persuasiveness that match the penetration and balance of his thought. Sir Walter, indeed, disclaims some of the credit for the book, attributing it in part to a conference of between thirty and forty university teachers of Christian outlook held at Cambridge in September 1946, where discussion centred largely on a series of "University Pamphlets" published by the Student Christian Movement Press (see *Nature*, **159**, 392; 1947). If, however, to some extent the book does express the converging thoughts of those and of many other minds, its value is derived none the less largely from the quality of Sir Walter's own thought and the lucidity of his prose. It is an outstanding British contribution to constructive thought about the future of the universities, and should be read by all those who have studied the latest report from the University Grants Committee itself, entitled "University Developments, 1935–1947" (see *Nature*, **163**, 154; 1949).

Sir Walter's book is, indeed, a natural complement to that report. With the single exception of the question of numbers, which bears so vitally on the maintenance of standards, Sir Walter probes deeply into all the issues raised in that report and into others less readily discussed in an official report. Broadly, the book is based on the conviction that much that ails universities to-day is closely connected with what is wrong with the whole world, and that the malady is rooted chiefly in the underlying assumptions, largely unconscious, by which their life and work are determined. The older universities, Sir Walter points out, grew up in a world very unlike our own. Their traditional assumptions are, to some extent, outdated and, in practice, discarded; but no agreement can be found on the answer to the questions how far this process should continue and what alternative assumptions should take the place of the old. The debate of the last ten years has revealed discord as well as vagueness concerning the *raison d'être* of universities and the standards to which they should pay allegiance and by which their policies must ultimately be judged.

Thus far Sir Walter would doubtless command general agreement; but he admits frankly that his argument and conclusions will only be thoroughly convincing to those who accept certain assumptions on which that argument is based. He postulates first that we are living in an age of exceptional crisis and that the issue depends chiefly on the human factor—in the last resort on the beliefs and sentiments which really govern the actions of men. These beliefs are in flux, and our society has lost direction.

* The Crisis in the University. By Sir Walter Moberly. (Published for the Christian Frontier Council.) Pp. 316. (London: S. C. M. Press, Ltd., 1949.) 15*s*. net.

The clue to reconstruction, he postulates, is to be found within our own tradition, and in that tradition the Christian element is vital, although the whole tradition needs to be purged and enriched.

The ability and willingness of the universities to respond to the challenge of the stupendous intellectual task with which, on these assumptions, our generation is confronted, are Sir Walter's primary concern. With these postulates in mind in successive chapters, he reviews the changing conceptions of the task of a university, and some causes of our present discontent. What he regards as the spurious remedies of scientific humanism and classical humanism are then examined and dismissed as inadequate—the latter, perhaps, a trifle too naïvely. Sir Walter then examines the fundamental questions of the aims and basis of a university, and ranges widely and with penetration over the varied and complex problems involved in academic freedom and integration, the content of studies, corporate life, the place of the university in the world, the task of Christians in a modern university and the role of a theological faculty.

This penetrating analysis thus covers the whole field of the functions and work of the universities. But the fundamental issues are always kept firmly in mind; there is no straying into irrelevancies, although Sir Walter writes with wisdom and discernment on many secondary questions as well as on his main theme. If the question of numbers is eschewed it is deliberate, because the number of students, and even what faculties should be maintained, are of secondary importance to that of determining the prime purpose of a university and what the community should expect to find in the new graduate. With this discernment go a balance of judgment, a persuasiveness and charm and a reasonableness that will commend the book even to those least disposed to accept the author's premises in their entirety.

Summarizing the results of this analysis and argument in a final chapter, Sir Walter suggests that while in technical advances the universities are showing the way, only to a slight extent are they protagonists in the transmutation of values. No accepted ideal has replaced the traditional one of training students for leadership in a stratified society now disappearing and in which we no longer believe. The universities have no agreed criteria by which to assess their policies, organisation, teaching methods and the forms of their communal life; and the crisis in the universities of which Sir Walter writes arises out of the contrast between the need of the time and present academic inhibitions and disabilities.

The accuracy of Sir Walter's diagnosis has since been emphasized in the presidential address of Lord Southway to the Classical Association delivered in Manchester on April 21. Lord Southway directed attention to the dwindling number in recent years of those distinguishing themselves in the Classical Tripos at Cambridge or in the School of Literæ Humaniores at Oxford who have found their way into political life or have attained political eminence. In this respect they are greatly outnumbered by the natural scientists, economists, psychologists, technologists and other kinds of experts. For all its priceless value, a higher classical education appears to be making little direct contribution to the daily business of the modern world.

What needs to be remembered is that in the last two generations the centres of political power have shifted, and leadership in Britain is no longer confined to such fields as the Church, the professions, the Civil Service and the armed forces of the Crown. At the same time, there is little evidence that the classical scholar is entering new fields, such as the trade union movement, in which clear thinking and selfless leadership with the classical qualities of restraint and toleration are no less important than, for example, in the Church. The very fact that the universities are not ostensibly providing leaders of the requisite calibre in all the important fields of modern life should impel teachers and administrators to think deeply about the issues to which Sir Walter directs their attention.

Sir Walter will doubtless feel recompensed if his book awakens university teachers and administrators to greater concern about these issues, for even the Christian revival for which he himself looks must comprise, as he recognizes, not only recovery but also an element of discovery. The positive policy he advocates starts with the removal of all inhibition of discussion of the vital questions of the day. The university must be a battle-ground where the real intellectual issues of our time are fought out, and their protagonists are confronted with one another. In such wrestlings the university must stimulate the characteristic academic excellencies of precision, comprehensiveness and candour, and the penetration which discerns the principles underlying practical disagreements. Moreover, such debates must be conducted at the level at which our several basic acceptances begin to diverge. This demands the restoration of communications between the isolated mental worlds which different groups have come to inhabit.

Even thus far, Sir Walter will doubtless stimulate much discussion; and to adopt his proposals might well shock the governing bodies of certain universities, as Bruce Truscot's books have indicated. But Sir Walter is emphatic that there is a limit to neutrality. There are issues so fundamental that real neutrality is impossible. Irrespective of internal differences, a university as a whole must stand for such basic academic values as a passion for truth, thoroughness in pursuing it to the bitter end, a delicate precision in analysis, a judicial temper, a willingness to learn from all quarters and an uncompromising insistence on freedom of utterance. There are also other values common to the whole community in which the university is set: for British universities these include recognition of an absolute moral obligation, an ingrained respect for law and order and an unshakable conviction that 'people matter'.

Beyond this, Sir Walter insists that in the university it is necessary constantly to explore and probe such values and to discover how far they are genuine and coherent. "It is the special vocation of the university to think things out and to trace particular truths back to the principles on which they rest." While,

however, he urges that a university should not be colourless or merely neutral on major issues, but should honestly avow the scheme of values by which the ordering of its life and studies is determined, his contention that these values can and should be increasingly Christian values does not lead him to claim for the Christian in the university more than the role of a creative minority.

This brief summary of Sir Walter's main argument may show the wide field he traverses, although it cannot do adequate justice to the persuasiveness with which it is presented or its lucidity and cogency. The argument also flows into numerous other questions, such as the regional responsibilities of a university, in which he firmly believes, the autonomy of a university and its relation to the State. A word must, however, be said of his treatment of the question of studies in relation to the equipment of a student to choose ends and recognize values so as to live his life more purposefully; this is necessary to show that what Sir Walter advocates is not that the universities should train their students with any particular philosophy of life—even a Christian philosophy—but that the students should be helped to formulate their own, and given the best guidance as to where the most effective help may be found.

Sir Walter's solution to the problem of combining general with occupational education involves breaking down the present isolation of subjects and departments, not only giving more attention to fields in which they overlap but also bringing different disciplines to bear on the same problem. The overloading of curricula must be ruthlessly assailed, and for members of staff a new balance between teaching and research must be found. A healthy insistence on discovery and original thought must not, he insists, be over-stressed to the virtual exclusion of any real intimacy between teachers and pupils, while customary methods of teaching and of testing must be reconsidered.

In discussing the way in which the university could adapt and revitalize the aristocratic tradition internally, Sir Walter just touches on the question of selection. Here he recognizes even more explicitly than the University Grants Committee in its report that there is, and must be, a place for others than those who have gained awards or scholarships, and he quotes pertinently from Lord Balfour's experience at Cambridge. Sir Walter shares the belief that A. Liddell Hart proclaimed in a recent broadcast on the Third Programme of the B.B.C.: "The closed shop in any form is the very antithesis of a living university". Liddell Hart went on to suggest that the Free University of Berlin may be worth watching "to see whether the German tradition of learning can be freely reconciled with the American development of the community centre, whether the German belief in an élite can be joined to the American faith in ever-widening opportunities".

Sir Walter has made a contribution to the discussion on the place and future of the university which is important not only in its British setting but also in the wider world context with which the United Nations Educational, Cultural and Scientific Organisation is already concerned. If he raises many controversial issues, he asks many questions to which answers must assuredly be found, and not least he challenges the complacency with which some popular ideas of the functions of a university and of university practice are accepted. The book is more constructive than Ortega y Gasset's "Mission of the University", which although equally challenging was more limited in scope; and Sir Walter is as uncompromising as is Sir Charles Grant Robertson in his belief that the universities must face the issue of religion if they are to become real laboratories and power-houses of spiritual and moral efficiency.

KINEMATIC RELATIVITY

Kinematic Relativity

A Sequel to "Relativity, Gravitation and World Structure". By E. A. Milne. (International Series of Monographs on Physics.) Pp. vii+238. (Oxford: Clarendon Press; London: Oxford University Press.) 25*s*. net.

THIS book contains a systematized account of the developments in kinematic relativity since the publication in 1935 of the author's "Relativity, Gravitation and World Structure". It is complete in itself, containing a sufficient account of the earlier work to enable it to be read independently.

To express opinions about the merits of kinematic relativity is to risk entering a domain of controversy relatively unfamiliar to the pure mathematician. Yet it is appropriate that the book should be noticed by pure mathematicians, to whom, as the author notes, it may have a greater appeal than to the "hard-headed experimental physicist". I have personally found it very attractive, with its clarity of statement, its careful attention to definitions and its logical development; but it is this same abstract approach that makes the theory unattractive to the empirically minded, and partly accounts, no doubt, for its incomplete popularity. Kinematic relativity has been the object of much criticism, some based apparently upon prejudice and some upon misconceptions of Milne's purpose and point of view; but it has fortunately also received a share of the respectful and dispassionate criticism due to a striking and powerfully argued theory.

Milne maintains that kinematic relativity is self-consistent, and that, being so, it is on a footing similar to that of the various systems of geometry, the acceptance of which bears no relation to their applicability to the external world. That may be so: but it is as a cosmological theory, and not as a mathematical *tour de force*, that it will almost certainly be judged, however great may be its intrinsic merits. Those merits are considerable, for, if the book is nothing else, it is a work of art, with a sense of intellectual adventure running through it.

Kinematic relativity is not concerned with the construction of different sorts of universes suitable for the representation of different phenomena, and so has no counterpart of the various worlds (flat, Schwarzschild, expanding, etc.) of general relativity. It is concerned with the construction of a single universe, idealized in the sense of being 'smoothed out', but possessing the main features of the physical

world. It is the single stage upon which the whole physical drama is to be played, and Mach's principle (that the contents of the universe determine its laws) is one of its corner stones.

The book is divided into four parts, dealing respectively with kinematics, dynamics, gravitation and electrodynamics. Much of the first part, which contains an account of the now almost classic theories of time-keeping and equivalence, is undoubtedly of lasting value, whatever the ultimate fate of the theory as a whole, for it makes a notable contribution to our understanding of the meaning of time in physics. From it emerges the basic picture of the kinematic universe, a universe populated homogeneously by equivalent particle-observers possessing a common system of time-keeping, their congruent clocks being capable of agreed regraduations. This idea of obtaining alternative descriptions of the universe by the adoption of different time-scales is of profound interest, and has a significance more fundamental than the changes of time co-ordinates (usually physically undefined) permitted in general relativity. The set of particle-observers (the 'substratum') is the idealized representative in Milne's abstract world of the system of galaxies of the actual universe.

The passage from kinematics to dynamics is claimed, remarkably but not unreasonably, to be the only logical possibility, no appeal being made to empirical physical laws. The rest of the book abounds in striking results and bold assertions, most of them already known to relativists through their earlier publication in research papers. Only brief mention of a few of them can be made here. Milne deduces, for example, that the gravitational γ and Planck's h vary with the epoch; he claims to have resolved Hubble's difficulties in interpreting the red-shifts; he assumes no electron spin, his electrons being point-singularities, but deduces for them an effective radius equal to that of the classical electron; and he obtains, perhaps most remarkably of all, an equation for the spiral arm of a nebula, which possesses the unexpected property of turning back on itself after a certain number of convolutions, thereby suggesting a 'trail'. How far his results (assuming them applicable to the actual universe) are susceptible of observational test, I am not competent to say: nor do I feel able to venture opinions about the philosophical implications of his ideas. But I am sure that the work is too well reasoned to be lightly dismissed, and I would recommend it to the serious attention of physicists as well as to mathematicians.

H. S. Ruse

FARM MACHINERY

Machines for the Farm, Ranch and Plantation

By Arthur W. Turner and Elmer J. Johnson. Pp. xvi+793. (New York and London: McGraw-Hill Book Co., Inc., 1948.) 36*s*.

THE authors of this book have set themselves the difficult task of providing information on the host of different machines used in American agriculture. They have made a good start by adopting an intelligent system of classification of the equipment under the six headings of seedbed preparation machines, crop planting machines, crop tillage machines, harvest and harvest handling machines, mechanical power and transportation machines, and general service machines and barn and produce equipment. The chapters are well set out with clear headings to the sections and sub-sections, a wealth of good illustrations and with a useful summary at the end of each; in fact, the plans and foundations are excellent. It is unfortunate that the structure built on those foundations is not nearly so good.

Each machine is dealt with from three aspects—its selection, its operation and servicing, and its reconditioning. The part dealing with selection consists mainly of a description of the different available versions of each machine and of the extra fittings or attachments that go with it. The direct advice that the authors give to anyone faced with the job of choosing a machine is usually confined to a few brief and relatively uninformative sentences. For example, their advice to someone choosing a field cultivator is to "Select a size to match the power and with teeth or sweeps best suited for the purpose used". They do not, however, give the reader any idea of the amount of power that field cultivators need, nor do they describe the performance or applications of the various patterns of teeth or sweeps. The authors have, in fact, dodged one of the major problems facing farmers to-day—that of finding a matching set of equipment made up of units that will work smoothly together, and will allow all the farming operations throughout the year to be carried on with a constant and minimum amount of labour. The advice given on the operation, servicing and reconditioning of farm machinery is more complete. The practical farmer and tractor driver would probably have preferred, in addition to the details of setting and maintenance, more information than is given on the technique of working the various machines in the field, while the farm mechanic might ask for more practical instructions on the overhaul and repair of the machines described.

This book, which at a first glance appears to be so useful to everyone connected with agriculture, is disappointing. It gives a very good picture of the range of farm machinery available in the United States, and will be of more use to students and lecturers than to farmers and mechanics, or to those engaged in research in the field of agricultural engineering.

J. C. Hawkins

CHEMICAL ESTIMATION OF WATER

Aquametry

Application of the Karl Fischer Reagent to Quantitative Analyses involving Water. By John Mitchell, Jr., and Dr. Donald Milton Smith. (Chemical Analysis, a Series of Monographs on Analytical Chemistry and its Applications, Vol. 5.) Pp. xi+444. (New York and London: Interscience Publishers, Inc., 1948.) 48*s*.

IN the whole area of chemical analysis few estimations are more important than that of moisture, and few in the past have received such casual treatment. That a volume of this size and character should be devoted substantially to one chemical method of estimating water is welcome evidence of the growing importance attached nowadays to precision in this field. The pendulum may, indeed, have swung a little too far; occasionally in this book

the enthusiasm of the authors for their subject and for the work of Karl Fischer approaches the mystical. The word 'aquametry', too, is awkward on the tongue and strange to the dictionary. Its coinage may be thought to imply that a new sub-branch of science has been established within narrow limits, and hence may disturb more than the linguistic pedants.

But these are defects of qualities; the chemist who is faced with the problem of determining very small quantities of water, or who finds in some special field the more traditional methods to be inadequate, will find this book a mine of information and a stimulating fountain-head of ideas. Theoretical backgrounds are not forgotten; but the emphasis is laid on working details, and specimen results are given for methods of estimating water in amounts varying from parts per million to high percentages and in materials selected from the whole range of organic and inorganic solids, liquids and gases. A host of difficulties are mentioned, discussed and, for the most part, surmounted with ingenuity and without sacrificing the distinctive advantages of the titration method of analysis for water.

Moreover, the precise determination of water as a chemical entity opens new horizons to the analyst. Many organic reactions result in the stoichiometric production or utilization of water, and by the use of the titration procedure the progress of such reactions may be directly followed. Largely as the result of the efforts of the authors and their co-workers, various catalysing solutions have been developed especially suitable for use with the Karl Fischer technique; for example, the use of boron trifluoride in acetic acid enables the esterification of alcohols to proceed rapidly to completion, and the amount of water so produced is a measure of the alcohol which has reacted. The estimation in this way of ethanolamines and other aliphatic amino alcohols in the presence of ammonia and amines may prove a most useful procedure.

By an ingenious inversion of the method, aliphatic acids can apparently be determined in the presence of esters and even of inorganic acids other than sulphuric, while aldehydes and ketones can be determined by titrating the water produced by the reaction with hydroxylamine hydrochloride. Some idea of the versatility of the application may be afforded by mentioning that it is suitable for determining nitriles, anhydrides, dibenzyl ether linkages in phenolic resins, and sulphur trioxide in fuming sulphuric acid, full working details being given for each example. Indeed, the authors are to be congratulated on the care with which they describe the apparatus and the preparation of the reagents employed, care which will be appreciated by the chemist handling for the first time solutions such as that of boron trifluoride in glacial acetic acid.

A final chapter is devoted to proposed further studies involving the Karl Fischer reagents. Among the more urgent of such studies, and one of which the authors are aware, is the problem of obtaining more stability in the main reagent itself. A slight impression of hurried work obtrudes itself toward the end of the book, exemplified by such instances as the submission of methods without any evidence that they have been tried. A few errata occur, of which the occasional confusion between benzine and benzene is perhaps the most serious; and the position of Chapter 2 may be found disturbing. It deals with practical details better left until the theory of the subject is mastered, and although in a footnote the authors state that the chapter is intended to be used as a ready reference and only by those who have familiarized themselves with the procedures given later, logic would surely have placed it in an appendix. Nevertheless, the book, which has copious references and comprehensive indexes, can be confidently recommended both for the library shelf and as a working manual.

C. F. M. Fryd

HOBHOUSE MEMORIAL LECTURES

Hobhouse Memorial Lectures, 1930–40

Pp. vii + 298. (London: Oxford University Press, 1948.) 10*s*. 6*d*. net.

THIS volume contains the first ten lectures delivered in memory of L. T. Hobhouse. They make an unusually interesting collection. The subjects selected are all clearly related to his interests, and most of the lecturers were personal friends. Hobhouse is chiefly remembered for his social and political studies; but two of the lectures indicate appropriately his contributions to the general theory of knowledge. Dawes Hicks contributes a characteristically penetrating but difficult discussion on "Thought and Real Existence"; C. S. Myers, under the provocative title "The Absurdity of Any Mind–Body Relation", argues cogently that the usual mode of discussing the relation involves a misconception of the nature of mind and body. J. L. Stocks's lecture on "Materialism in Politics" starts from the Aristotelian argument that where there is form or order it is an error to suppose the form is produced by the material which displays it. His point is that this error is common in modern political thought; not only among Marxists, but also among many who consider themselves good democrats after the Western model, to the detriment of democratic institutions.

Three lectures may be grouped together as definitely sociological. J. A. Hobson, who gave the first of the series, under the title "Towards Social Equality", discusses the nature of equality and the risks accompanying it. K. Mannheim, on "Rational and Irrational Elements in Contemporary Society", unfortunately conceals valuable thinking under obscure terminology. Prof. M. Ginsberg, on "The Unity of Mankind", deals in a most interesting way with the possibilities and difficulties. Two are definitely factual: Dr. J. L. Hammond gives an admirable account of "The Growth of Common Enjoyment", one of the genuine social advances of the past hundred years; Dr. R. H. Tawney's lecture, "Some Thoughts on the Economics of Public Education", deals with the state of affairs in the 1930's, and is interesting to read now in view of recent changes.

Lastly, there are two that are hard to classify: Prof. H. Laski, on "The Decline of Liberalism", indicates what he considers theoretical defects in liberal thought and certain failures in practice. Whereas Prof. Laski is considering two countries, Great Britain and the United States, on a time-scale of two centuries, Prof. A. J. Toynbee surveys the whole world on a scale of millennia in his lecture on "The Downfall of Civilizations". His diagnosis of the state of our present civilization is that the trouble is chronic rather than acute, and that with care the patient may live for a long time.

A. D. R.

Diseases of Potatoes, Sugar Beet and Legumes

By Dr. W. A. R. Dillon Weston. Pp. viii + 86. (London, New York and Toronto: Longmans, Green and Co., Ltd., 1948.) 4*s*. net.

THIS book was written for farmers and students, and it seems also to be suitable for senior classes in schools. The information is well chosen and reliable, and it is presented in such a way that non-scientific readers can, with a little perseverance, obtain a sound working knowledge of the nature and treatment of the diseases considered.

The 'pictorial strip' method of illustrating the life-histories and treatments of pests and diseases may be said to be Dr. Dillon Weston's invention, and readers of *Agriculture* are already familiar with its uses. Some of these illustrations are delightful, and they do achieve, to a great extent, their object of providing a complete 'bird's-eye' view of each problem; but the individual pictures are frustratingly small, and the minute text accompanying them introduces many scientific names and special terms, which are doubtless useful to students but might easily prove to be sources of irritation—to farmers of the older school, for example. This effect is also produced, at least on the present reviewer, by the rather lavish use of the rhetorical question; there are nine between pages 7 and 15.

The diseases described are mostly of known economic importance. A few of less importance, such as sugar beet mosaic virus, are given brief reference. Mention might have been made of the virus diseases of legumes. Although little is known of their economic importance in Great Britain, they are very common, and must cause some damage to crops, such as field peas. There is a danger that such damage might be wrongly attributed to fungus attack or nutritional disorders. In general the ground has been very well covered, and this will be a most useful book. M. A. W.

The Lungfish and the Unicorn

An Excursion into Romantic Zoology. By Willy Ley. Pp. 254. (London: Hutchinson's Scientific and Technical Publications, 1948.) 10*s*. 6*d*. net.

NOTHING captures the imagination of man more than the thought that somewhere, hidden behind the green, moist walls of almost impenetrable jungle, lurks a reptilian survivor of the prehistoric past. In this well-documented and intelligent book Mr. Willy Ley discusses not only the popular rumours and the 'living fossils' such as the tuatera, platypus and lungfish, but also the historical and mythological background to the strange creatures of legend and antiquity.

Though but a small book it contains a fund of interesting data upon the weird and the wonderful. The unicorn, dragon and sea-monster, etc., are discussed, together with the literary and scientific foundations of the belief in giant men.

One of the most interesting chapters is "The Dragon of the Ishtar Gate", which deals with many odd tales brought back by seamen and explorers concerning the dragon. Mr. Ley believes that it is not impossible for the *Brontosaurus*, or some similar Mesozoic saurian, to exist to-day in central Africa—an interesting theory.

It would be, of course, impossible to include everything in a book of this size; but it is somewhat surprising to find reference to neither the Norwegian kraken nor to the 'abominable snow-men' of the Himalayas—two enigmas that have perplexed the world for some considerable time. However, on the whole "The Lungfish and the Unicorn" is an excellent book, written in an easy, conversational style and capturing the interest throughout.

The illustrations, to some of which a full page might well have been devoted, are taken either from the classics or from later zoologies, while some were specially drawn by Prof. Gustav Wolf and are very suited to a work of this nature.

Animals We Use

Written and illustrated by Arnrid Johnston. Pp. iv+32. (London: Methuen and Co., Ltd., 1948.) 10*s*. 6*d*. net.

IT has been a common complaint since the teaching of biology became widely accepted in schools that many pupils have acquired a considerable knowledge of the insides of the 'types' prescribed for dissection, but have remained largely unaware of those animals and plants the evolution of which has run close to that of man. The late Sir D'Arcy Thompson, for example, often lamented that, after high promise, biology as a school subject had descended to a level in which pupils were being taught much about the morphology and physiology of local species, but were learning little of the habitat and habits of the larger animals throughout the world.

The appearance of Arnrid Johnston's book is, therefore, much to be welcomed, and, with its thirty-two pages of lively coloured drawings, should be most useful to young and old by showing the contribution animals have made throughout the ages to civilization all over the world both because of their co-operation with men and because of their innumerable marketable products. Much hard work has gone into the compilation of this book, and the explanatory captions provide an effective complement to the attractive drawings. The book should prove an admirable partner to the author's previous work, "Animal Families and Where They Live". T. H. H.

Akim-Tokotu

An Omen of the Gold Coast. By Dr. M. J. Field. Pp. viii + 211 + 23 plates. Accra Government Printing Department. (London: Crown Agents for the Colonies.)

DR. M. J. FIELD, the author of two valuable works on the Ga people of the Gold Coast territory, was employed by the Gold Coast Government in 1938 to investigate the native system of State finance and of allegiance and jurisdiction in one of the confederations of the Western Akim peoples. She builds on the work of Rattray and others, who have described the political and legal system of the Akan peoples to the south-east of the Akim area, who have similar customs; but her own study is an intensive investigation of particular areas written from personal observations made in some seventy towns. She writes clearly and well, and her work is always stimulating. She has valuable material to give on the origin of these towns, their finances and their system of land tenure. She also gives accounts of kinship usages, marriage, inheritance and succession, and some of the traditional ritual, as well as new cults at fetish shrines. The result is a very useful extension to our knowledge of the peoples of the Gold Coast. It is a pity that the material was held up from publication for so long and has been apparently so severely censored by the Government, particularly the sections on health.

SOVIET GENETICS: THE REAL ISSUE

By Dr. JULIAN HUXLEY, F.R.S.

NOW that the long-drawn-out dispute over genetics in the U.S.S.R. has come to a close, with the complete defeat of the neo-Mendelians at the hands of Lysenko, it is time for men of science outside the U.S.S.R. to take stock of the situation and to see what implications and consequences it has for them. I believe that the situation is very grave. There is now a party line in genetics, which means that the basic scientific principle of the appeal to fact has been overridden by ideological considerations. A great scientific nation has repudiated certain basic elements of scientific method, and in so doing has repudiated the universal and supranational character of science.

That is the major issue. Its discussion has been unfortunately clouded by insistence on subsidiary, minor, and sometimes irrelevant issues. In relation to this main issue, it is subsidiary whether or not Lysenko's claims to have made certain new discoveries are substantiated, and whether his theories are wholly or partly sound. It leaves the main issue untouched if the attempt is made to justify the action taken, on the narrowly practical ground that the agricultural production of the U.S.S.R. must be rapidly increased, or on the more general ground that Marxism must believe in the improvement of the environment and must or would like to believe that such improvements have a permanent effect on heredity. It is of no relevance to the main issue that Mendelism has sometimes been used to justify undesirable theories and actions, such as Nazi racialist theories or the exaggerated theories of inherent class superiority put forward by certain eugenists. It is equally irrelevant that Mendel was a Roman Catholic priest, or that this or that noted geneticist was a political reactionary. It is a subsidiary issue that some geneticists in the U.S.S.R. may have been directly or indirectly 'liquidated'. It is confusing the real issue to recall that in a wholly or partly planned economy the State must decide how money should be spent on scientific research and its application; or that men of science outside the U.S.S.R. cannot always obtain official grants for the researches they want to undertake, or always get their papers accepted for publication; or that capitalist as well as communist countries insist on secrecy for certain kinds of research and deny free publication to their results. All these issues are, I repeat, either irrelevant or merely subsidiary to the major issue, which is the official condemnation of scientific results on other than scientific grounds, and therefore the repudiation by the U.S.S.R. of the concept of scientific method and scientific activity held by the great majority of men of science elsewhere.

To make the issue clear, I will begin by quoting from the report of the proceedings of a meeting of the Praesidium of the U.S.S.R. Academy of Sciences of August 26, 1948 (ref. 3; p. 663)*, the highest and most powerful scientific authority in the land, to which the issue after previous debates by the Agricultural Sciences (see later) was referred. Here and elsewhere I have italicized passages which seem to me particularly relevant. When passages are from verbatim translations, I have given them in double quotes (". . ."); when from summarized reports, in single quotes ('. . .').

The Praesidium of the Academy of Sciences passed twelve resolutions. Of these the most important for our purpose are the following (the translation has been slightly condensed):

'(3) The Cytogenetical Laboratory of Cytology, Histology and Embryology headed by N. P. Dubinin, shall be abolished *as unscientific and useless*. The Laboratory of Botanical Cytology at the same institute shall be closed down *on the grounds that it has followed the same incorrect and unscientific line*. . . .

'(4) The Bureau of the Division of Biological Sciences shall be charged with the preparation of plans for scientific research work for the years 1948–50. *In this the Bureau shall be guided by Michurin's teaching*, and shall adjust the scientific research work of biological institutes to the needs of national economy.

'(6) The composition of Scientists' Councils at biological institutes and editorial boards of biological publications shall be checked with the object of removing from them *the partisans of Morgano-Weismannite genetics* and of replacing them by *supporters of progressive Michurinite biology*.

'(7) The Division of History and Philosophy shall be charged with inclusion in its programme of popularization of the achievements of Michurinism and of *critical exposure of the pseudo-scientific Morgano-Weismannite tendency*.

'(11) The Bureau of the Division of Biological Sciences shall revise the syllabuses at biological institutes, *bearing in mind the interests of Michurinism*.'

An explanatory statement follows, including the following remarks: 'At a number of Academy institutes *formal genetics has not been combated with sufficient vigour*. For this the Praesidium of the Academy takes the blame. The Bureau of the Division of Biological Sciences and its head L. A. Orbeli (who was released from his duties as Academician-Secretary under Resolution 1) have failed to give a correct orientation to the biologists of the Academy.

'The report by Lysenko (1948; and ref. 3), which has been approved by the Central Committee of the Communist Party, *has exposed the scientific inconsistency of the reactionary idealist theories of the followers of Weismannism*—Schmalhausen, Dubinin, Zhebrak, Navashin and others.'

A letter to Comrade Stalin is summarized as follows: 'A pledge is here given by the Praesidium of the U.S.S.R. Academy of Sciences to further Michurin's biology *and to root out unpatriotic, idealist, Weismannite-Morganist ideology*'.

A further statement by the Praesidium ("To the prosperity of our progressive science") is finally summarized: 'Michurin's materialist direction in biology *is the only acceptable form of science, because it is based on dialectical materialism and on the revolutionary principle of changing Nature for the benefit of the people. Weismannite-Morganist idealist teaching is pseudo-scientific, because it is founded on the notion of the divine origin of the world and assumes eternal and unalterable scientific laws. The struggle between the two ideas has taken the form of the ideological class-*

* See list of literature cited. Figures refer to summaries and reports; individual contributions are cited alphabetically by author and date.

struggle between socialism and capitalism on the international scale, and between the majority of Soviet scientists and a few remaining Russian scientists who have retained traces of bourgeois ideology, on a smaller scale. There is no place for compromise. Michurinism and Morgano-Weismannism cannot be reconciled.' (Note here, among much else of interest, the apparent distinction between *Soviet* and *Russian* scientists.)

There are also now available Lysenko's "Report on Soviet Biology" to the session of the Lenin Academy of Agricultural Sciences, July 31–August 7, 1948, together with his concluding speech (Lysenko, 1948; and in ref. 3), two summaries of the subsequent discussion[1,2], and verbatim reports of a few of the speeches[4-9]. Since this article was first written, a verbatim English translation of the entire discussion, totalling 631 pages, has been published in Moscow[3]. Neither space nor time has been available for the general use of verbatim extracts from this, but I have satisfied myself as to the general accuracy of the summarized citations given below.

These reports, together with the documents already cited, constitute a melancholy landmark in the history of science. They demonstrate that science is no longer regarded in the U.S.S.R. as an international activity of free workers whose prime interest it is to discover new truth and new facts, but as an activity subordinated to a particular ideology and designed only to secure practical results in the interests of a particular national and political system. Consequently the unity of science is denied, and various brands of "good" science—Marxist, Soviet, or materialist—are distinguished from various brands of "bad" science —bourgeois, reactionary, idealist and the like. Further, the primary sanction for scientific theory is no longer consonance with the facts of Nature, but consonance with a political and social philosophy. With this, orthodoxy is once more enthroned; and though this is no longer the theological orthodoxy from the bonds of which the Western world emancipated itself in the seventeenth, eighteenth and nineteenth centuries, the new social-political orthodoxy is equally powerful, employs abuse and force in a similar way, and is equally inimical to the free spirit of science. There is now a scientific party line in the U.S.S.R., and those who stray from it do so at their peril.

It is true that up to the present this complete subordination of science to political authority applies only in genetics. However, tendencies in the same direction have also manifested themselves in the U.S.S.R. with more or less force in other fields of creative and intellectual activity—philosophy, literature, the visual arts and even music—and in other scientific subjects, such as psychology and the theory of probability. Further, once the principle of a dominant orthodoxy has been admitted and acted upon on one field, it can readily be generalized, and the presumption is that it will be. In any event, there can now be no security that other branches of science in the U.S.S.R. will not suffer the same fate as genetics, and be *gleichgeschaltet* in relation to an overriding system.

Let me illustrate these points by quotations, at the same time trying to imagine what would have happened if the controversy had developed in Britain or other centre of "bourgeois science".

In the first place, the two sides have been elaborately labelled, and many of the labels have philosophical or political connotations, often implying approval or condemnation. Thus, neo-Mendelism is usually referred to as *Morgano-Mendelism*, often with one or more of the adjectives *formalist*, *idealist* or *reactionary* prefixed; or simply as *idealist genetics*. Sometimes it is styled 'Weismannism' (again usually with a pejorative prefix), although in the West, Weismann's particular views are now mainly of historical interest only. We are told that the views of the neo-Mendelians are *mystic*, *metaphysical*, *bourgeois*, *pseudo-scientific*, or even *anti-scientific*.

The followers of Lysenko, on the other hand, are called *Michurinites*, presumably because the Soviet tendency is to justify the present in terms of past authority, and Michurin is being deliberately glorified as a great Russian pioneer in agricultural and biological science (whereas in point of fact he was essentially an empiricist who scored some important practical successes, but whose theoretical speculations have become scientifically negligible in the light of later research). Timiriazev is also often cited as an authority under whose banner Lysenko and his followers are advancing (although his genetical theories are now quite outdated by scientific advance). Further, Michurinism is usually qualified with the adjective *scientific*, *materialist*, or *progressive*. The term *Soviet genetics* is not infrequently used, and Lysenko employs the phrase *Soviet creative Darwinism*.

Subtlety of description is pushed to extremes by B. M. Zavadovsky, who distinguishes between *Mendelism* as a system of established facts, and *Mendelianism* as Mendelism distorted by reactionary idealist and metaphysical elements. (But Zavadovsky was in a very awkward position, as an ardent and important member of the Communist Party who had for several decades done a great deal to popularize neo-Mendelism.)

In Britain, I suppose we should have heard simply of certain new claims of Lysenko which required confirmation, and of certain new theories of his which were in conflict with accepted views; or perhaps the matter would have developed into a general dispute between neo-Mendelians and Michurinites, in the same way that the quarrel between Karl Pearson and Bateson developed into a general dispute between biometricians and Mendelians some forty-odd years ago. Individual participants in the controversy might have been stigmatized as old-fashioned or uncritical; but there would certainly have been no wholesale attaching of philosophical, political or moral labels.

In the second place, the chief two touchstones in the controversy in the U.S.S.R. have been, not scientific fact and verification of theory by experiment, but immediate practical utility on one hand and correctness of doctrine on the other. The criterion of practical utility is probably of less general significance. In any event, it is not universally applied. I can testify from personal experience at the time of the

* It is to be noted that the summary in *Europe*[2] is somewhat tendentious, in that it does not merely give summaries of all the speeches, but groups them under headings, and summarizes what it considers relevant to each heading. Thus one of its headings is entitled "La Pratique Mitchourienne" and is editorially introduced as follows: "C'est un véritable bilan de victoires qu'a entendu l'Académie d'Agronomie quand défilèrent les savants et praticiens venus de tous les points de l'immense Union Soviétique pour témoigner des resultats positifs de la pratique mitchourienne." (As later pointed out, many of these successes are in point of fact due to the application of ordinary principles of selection, or to the unconscious utilization of Mendelian principles.) Only the parts of the speeches relevant to the success of Michurinism are summarized: other parts are omitted.

Another section is headed "Caractère Réactionnaire de la Génétique formaliste". The section devoted to the speeches of Zhebrak, Zavadovsky and others who defended neo-Mendelism is headed "la Génétique Formaliste".

It should also be mentioned that Waddington's articles (1948–49) omit all reference to what appears to me to be the real or at least the major issue.

celebrations of the Academy of Sciences in 1945, that there was in many fields, including ecology, genetics, systematics and general biology as a whole, an admirable balance between 'pure' and 'applied' work in the U.S.S.R., and that some branches of science with negligible practical applications, such as vertebrate palæontology, were extremely flourishing. However, in the case of genetics, the utilitarian criterion has been drastically employed—largely, I imagine, because the controversy has been so largely guided by Lysenko, and Lysenko is an agriculturist whose primary aim has been to achieve success through spectacular practical results.

Thus, in his "Report" Lysenko (1948) says: "Socialist Agriculture, the collective and State farming system, has given rise to a Soviet biological science, founded by Michurin—a science *new in principle* [italics mine throughout, unless otherwise stated], developing in close union with agronomical practice. . . . It is no exaggeration to say that *Morgan's feeble metaphysical 'science'* . . . can stand no comparison with our *effective Michurinist agrobiological science*."

Lysenko later refers to Michurin as "the great transformer of Nature", and says "in our country the Morganist cytogeneticists find themselves confronted by the *practical effectiveness of the Michurin trend* in agrobiological science". With reference to the special laboratory under Zhebrak, set up in the Timiriazev Academy by the Ministry of Agriculture, to study polyploidy (which, I may mention, has obtained some extremely interesting results), Lysenko merely says that, in his view, "it has produced literally nothing of practical value. Here is one example, . . . to show how useless is the practical and theoretical programme of our domestic Morganist cytogeneticists". Finally, in the conclusion of his "Report", he writes that "*a scientific handling of practical problems is the surest way to a deeper knowledge of the laws of development of living nature*" (italics his)—a sweeping assertion in obvious contradiction with many events in the history of science.

In the discussion, the same thesis is reiterated. Thus Nemchinov states that the task of agricultural science is to change Nature for the benefit of socialist economy (*nota bene*, not of humanity in general), and Lobanov says that Soviet agricultural science must 'aim at the successful solution of practical problems' (a statement with which no one would quarrel if it were not constantly extended to mean that all genetics must be directed *only* to the solution of practical problems). Some of the speakers even went further. Olshansky states that Morgano-Mendelism '*obstructs* the work of practical breeding and seed-growing'; and Dubinin's interesting study of the selective effect of environment on the genetic composition of a population of *Drosophila*, because it is of no immediate practical value, is described by Yakushkin as '*a monstrous deviation from the tasks of a Soviet scientist*'. Babajanyan, when asked by Rapoport why he shut his eyes to the existence of useful as opposed to deleterious mutations in *Drosophila*, answered (ref. 3; p. 163) "*because they are useful mutations for a useless object*". Previously he had said "Who wants what by their very nature are useless *Drosophilas*?" There could not be a clearer repudiation of the idea that one of the basic functions of science is to obtain knowledge and understanding. Dimitriev condemns all scientific work (in genetics) which does not assist practical agriculture, and criticizes Schmalhausen and others '*for expressing views incompatible with progressive improvement in agriculture*'. (Apparently, he regards it as irrelevant whether the views happen to be true or not.)

On the other hand, some speakers give the practical criterion a twist and assert that 'Morgano-Mendelism is a bourgeois philosophy *seeking nothing but the exploitation of Nature*' (Dvorjankin), and that 'self-pollination and selection of selfed lines of maize use Morganist techniques, which made seed-production difficult *and play into the hands of capitalist seed-firms*' (Feiginson).

One of the reasons given by the Academy for closing down the Cytogenetical Laboratory under Dubinin (see above) is that it is "useless". (The Academy must have forgotten Faraday's answer to a questioner who asked him what was the use of his work: "What is the use of a baby?")

In Britain, the practical utility of this or that discovery or the immediate applicability of this or that theory would doubtless have been discussed; but no one would have questioned the desirability of leaving a considerable free sector to pure research, whether on the two-fold ground, usually accepted here, that one of the aims of science is to increase knowledge irrespective of practical results, and that practical results do, as a matter of fact, often spring from what appear to be the most impractical investigations, or, in the case of a minority, for the latter reason only. More Government money might have gone into Michurinite work if the Government and its advisers had been impressed by Lysenko's claims; but it is safe to say that no laboratory turning out a considerable volume of research results would have been closed down as useless.

More central to the issue is the appeal to doctrine and authority instead of to observational and experimental verification. As a result, a basic effect of the controversy has been to establish, in the fields of genetics and evolution, a scientific orthodoxy, which in its turn is related to and dependent upon a philosophical orthodoxy. And the philosophical orthodoxy is, of course, linked with the social and political orthodoxy of Communism and the authority of the Communist Party in the U.S.S.R. The upshot is that science in the U.S.S.R. must now do its work in a totally different atmosphere and on totally different intellectual foundations from those in other countries.

The full extent of the change of attitude and atmosphere can only be properly appreciated by reading the actual proceedings of the session in detail. However, I will try to convey its essence by some extracts.

In the first place there is often a stress on the truth or falsity of "teaching" ("doctrine", "standpoint", or "trend") which recalls scholasticism rather than natural science*. Thus Perov says "*the only true trend*

* It is difficult to disentangle the various terms, since in different translations or summaries the same Russian words may be translated differently. However, two conclusions are quite clear. First, that the Michurinites equate the terms *theory* and *teaching* (see Lysenko's remarks, below); so that for them a scientific theory means something quite different from what it does to Western scientists, who regard it as a general framework of concepts giving a satisfactory interpretation of a mass of established facts and laws; and secondly that the various terms such as *teaching*, *trend*, *doctrine* and *theory* mean some body of ideas whose truth or falsity can be determined *a priori*, or at least on ideological grounds, rather than by testing against the facts of Nature.

Truth, like other general terms, can be employed in various senses. For Western scientists, "scientific truth" has to do with a process—the slow accumulation of tested facts and the growth of explanatory ideas providing insight and understanding of Nature. For Soviet scientists, it is clearly something more, since it is concerned with the underlying assumptions which science ought to make in relation to a dominant general ideology. One is tempted to add that it is more scholastic, and indeed metaphysical, although both these terms are applied abusively by the Michurinites to the views of their opponents.

in the science of biology is the Michurin trend". Nuzhdin gives three examples '*supporting Michurin's teaching*'. Lysenko himself in his "Report" writes: "The appearance of Darwin's *teaching*, expounded in his book 'The Origin of Species', marked the beginning of scientific biology. The primary idea in Darwin's theory is his *teaching* on natural and artificial selection". Lysenko himself in his concluding remarks[2] writes: ". . . for the first time in the history of biology[!] *a truly effective theory* [in French[2] *une véritable théorie scientifique*] has come into being—*the Michurin teaching*". Prezent[2] says "in order that the Morganists might be 'reconciled' with the Michurinian theory, *they would have to renounce every one of the theoretical concepts of their false doctrine*".

Finally, the resolution adopted at the session sums up the issue by saying: "L'Académie Lénine des Sciences Agronomiques doit devenir réellement un centre scientifique *pour l'elaboration approfondie de la doctrine Michourienne*"[2].

The manifesto referred to on p. 662 of the summary in *Plant Breeding Abstracts*[1], under the title of "Up the Flag of Progressive Michurinite Biological Science", goes further, in stating that Lysenko's "Report" '*shows* that *Michurin's teaching is materialist and progressive*, while *the teaching of Weismann, Mendel and Morgan is reactionary and idealist*'. Here, as in Perov's earlier cited remarks, we have the distinction between true and false doctrines or trends, a distinction occasionally over-emphasized by zealots, such as Belenky, who describes Darwin and Lamarck as *infallible* (in spite of Lysenko himself having pointed out numerous "errors" in Darwin's "teaching").

The truth or falsity of doctrine is, as already indicated, linked up with the relation of scientific theories to politico-philosophical orthodoxy. This theme recurs constantly throughout the controversy. Lobanov says that 'Morgano-Mendelism is *a bourgeois philosophy*'. Dvorjankin goes the whole hog, in throwing doubt even on the facts discovered by the upholders of false doctrine, for he says that '*too much credence should not be given to bourgeois science*'. Alikhanyan, one of the few practising Mendelians who spoke, while insisting that 'there is nothing idealist about chromosomes and genes', endeavours to show his conformity with doctrinal orthodoxy by adding that '*they must be considered in the light of dialectical materialism, and the chromosome theory must be purged of idealist elements*'.

As a corollary of all this, any defence of neo-Mendelism immediately becomes "propaganda" for the "false doctrine". Thus Kostryukova complains that Russian biological journals such as the *Journal of General Biology* or the *Comptes Rendus* (*Doklady, Reports*) *of the U.S.S.R. Academy of Sciences* have for some years published 'no articles with a Michurinite tendency'. Thus, 'thanks to this choice of articles, *Morganist propaganda* was being conducted in every part of our country. However, in future, as we have seen, *the editors of scientific journals are no longer to have the right to select what articles they think are scientifically valuable*'. That this is now the case is confirmed by the Minister of Higher Education himself (Kaftanov, 1948), who after referring to "the reactionary Weismann-Morgan theories", states that "our present aim is to fill the ranks of the teaching profession with Michurinist biologists".

Petrov says that 'present [Russian] text-books on genetics are full of *formal and unscientific nonsense*'. Glushchenko, Lysenko's chief helper, deduces the inheritance of acquired characters, not from experiment but from first principles, in a manner reminiscent of scholastic theology: '*Dialectics . . . show* that biological processes involve the principle of external action, and *that such external influences become internal properties after assimilation by the organism*'. The seventh resolution of the Praesidium of the U.S.S.R. Academy of Sciences charges the Division of History and Philosophy with "the critical exposure of *the pseudo-scientific Morgano-Weismannist teaching*".

Lysenko in his "Report" says that a major error of Darwin lay in his "transferring into his *teaching Malthus' preposterous reactionary ideas* on population". Later he writes "Reactionary biologists everywhere have . . . done everything in their power to empty Darwinism of its materialist elements"—a somewhat strange accusation to bring against the neo-Mendelians who have discovered and elucidated the material basis of heredity in the chromosomes, and in so doing appear to be on the track of the essential material basis of life itself.

This is a very sore spot with the Michurinites. Thus when Nemchinov was saying that he could not agree that "the chromosomes have no relation with the mechanism of heredity", there was a disturbance in the hall, and one delegate shouted "There are no such mechanisms". More important, Prezent, the philosophic interpreter of Lysenko, says that "nobody will be led astray by the Morganists' false analogies concerning the invisible atom and the invisible gene. *Far closer would be an analogy between the invisible gene and the invisible spirit.*" It seems incredible that a man who claims to be a philosopher could descend to such 'arguments'. It can only be because he has no serious arguments to put forward, and the facts are against him. The existence of genes and of atoms has been demonstrated in an essentially similar way, and the spatial arrangement of genes within chromosomes can be ascertained with an exactitude as great as that of atoms within molecules. Turbin (ref. 2; p. 141; translated rather differently in ref. 3) reveals his biological ignorance (or unwillingness to consider facts) in his conclusion: 'We reject the fantastic assertion of the Morgano-Mendelists, according to which, to deny the genes would be to deny the material basis of the phenomena of heredity'. On the contrary, he continues, it is the cell which must be 'considered as the material basis of the phenomena of heredity'—which is, of course, in a sense true, but so vague as to put biology back by eighty years.

Lysenko in his "Report" writes as follows: "Again, an example of how uncritically our Mendelist-Morganists accept *idealistic genetics* is the fact that the standard textbook on genetics in many of our colleges has until quite recently been a translated American textbook, by Sinnott and Dunn" (which, if not perfect, is certainly both competent and comprehensive). He even goes so far as to describe Morganism as a "*pseudo-science*".

In order to support his own Lamarckian or what one might call Michurino-Lamarckian views, Lysenko frequently states that the inheritance of acquired characters is *materialist*, whereas Weismannism and Mendelo-Morganism are *idealist*, *mystical*, *formalist*, *scholastic* and *metaphysical*. He even claims that "*the materialist theory of the evolution of living nature . . . is unthinkable without recognition of the inheritance of acquired characters*".

He asks for the complete exposure of the "Morganist metaphysics, which is in its entirety an impor-

tation from *foreign reactionary biology hostile to us*". Here we have the logical extreme of the subordination of scientific theory to a socio-political philosophy, in the shape of its subordination to nationalism. It becomes unpatriotic to believe in neo-Mendelian facts, because Mendelism is a product of men of science from bourgeois and capitalist countries. Thus Lysenko later says that in the U.S.S.R. "young scientists with an insight into questions of philosophy have in recent years come to realize that the *Morganist views are utterly alien to the world outlook of Soviet people*"—as if this, and not their concordance with the facts of Nature, were the criterion of their scientific validity!

Mitin is summarized thus: '*the reactionary, idealist Morgano-Mendelian* school of thought . . . is condemned', and its followers are called '*unpatriotic*'. Zhebrak, who had the temerity to publish an article in *Science*, criticizing Lysenko and his methods, was publicly criticized by the Timiriazev Academy for his "unpatriotic action"; and Nuzhdin says that Zhebrak's article showed that the neo-Mendelians in the U.S.S.R. were "ready to form a common front with the most reactionary bourgeois genetics". Prezent[2] refers to "*the corrupting influence of Morganism*" as having penetrated into non-biological circles in the U.S.S.R.

Unworthy motives are ascribed to foreign men of science. Thus Prezent, the philosophical *eminence grise* of Lysenko, referring to the painstakingly fair and almost ultra-conscientious appraisal of the work of Lysenko and his school by the two Englishmen, Hudson and Richens (1946), does not scruple to say that '*the motives of Hudson and Richens*, who suggest that Russian varieties may not be pure, *are . . . questioned*'.

Finally, the Praesidium of the U.S.S.R. Academy of Sciences itself, in a letter to Stalin[3], pledges itself 'to root out *unpatriotic, idealist Weismannite-Morganist ideology*'.

This preoccupation with teaching or doctrine and with an overriding philosophy leads logically on to two further results—the establishment of a party line in science, and the division of science into two camps. Thus the manifesto from which I have previously quoted (ref. 1; p. 662) also states that 'the Report by Lysenko, which has been approved by the Central Committee of the All-Union Communist Party (Bolsheviks), *lays down the party line in biology*'. Zhebrak, in a letter (summarized in ref. 1; p. 662), writes that he "finds it necessary to withdraw from his previous position [on genetics], *since this has been declared mistaken by the Central Committee of the All-Union Communist Party*".

The question of the unity of science clearly preoccupies the more philosophically minded Michurinites; but their claim that scientific work in a capitalist country must be tainted with bourgeois ideology, and to such an extent that its scientific value may be thereby vitiated, enables them to deal with it to their own satisfaction.

Listen once more to Prezent (ref. 2; p. 134; my translation from French)*: 'The opponents of the Michurinite trend in agrobiology and their allies have invited us to recognize the rational elements in *bourgeois biological science* and not to break our links with *so-called world science*. It is true that we Michurinites *do not have the same conception as the Morganists as to what classical biological science has bequeathed to us, nor as to what the Morganists call "contemporary world biological science". The content they give to this term is territorial, not theoretical. World biological science is represented by the most progressive science* [la science d'avant-garde] *at each stage of its history. To-day, the most progressive materialist biological science is the Michurinite tendency of Soviet agrobiology.*

'*Is it not extraordinary that in our country*, where dialectical materialism is the methodological basis of science, with an undeniably revolutionary character, *there appear from time to time dreams of the unity of our science with contemporary foreign science?*'

This at least is plain: we know now that the U.S.S.R. does not believe in the unity of science. But it is worth recalling that "world science" in any branch is represented not merely by its actual *avant-garde* tendencies, but also by the body of knowledge accumulated by past generations of scientific workers.

Prezent (ref. 2; p. 137) makes himself still clearer when he is treating of free scientific discussion. 'Nobody forbids and nobody can forbid discussion in science. Let us accordingly discuss *within the framework* [à l'intérieur même] *of the Michurinite tendency*, the best way of studying this doctrine, and applying it in practice. *But it is high time to demand from you your participation in the struggle against the theories of foreign biologists.*' (The translation in ref. 3; p. 602, runs: The Morganists "want a discussion. *But we shall not discuss with the Morganists* (applause); *we shall continue to expose them* as adherents of an essentially false scientific trend, *a pernicious and ideologically alien trend, brought to our country from foreign shores. (Applause.)*)" Here we have it in black and white. Free discussion—but within limits. No freedom for "foreign" theories except to be combated.

During the actual session, a few Mendelians were allowed to defend their views. But the general atmosphere was extremely hostile to them, and almost all were apologetic or tried to justify their views on the basis of Marxist principles. There had clearly been a marked tightening-up since the earlier pre-war congresses on genetics, when many more Mendelians spoke, and spoke much more freely (Muller, 1948; and see ref. 5).

Of course, when Lysenko announced that his views had the official approval of the Communist Party (ref. 3; p. 605), it was clear that the Mendelians were being allowed to speak merely to give a semblance of free discussion (as well, perhaps, as to give them a rope to hang themselves with). They have now all been dismissed or disgraced. Further, from now on (until official policy changes) there can be no further freedom of speech on genetics; neo-Mendelism is to be denounced and combated, not discussed.

If the discussion had taken place in Britain, I do not think that either philosophical, political or patriotic labels would have been attached to the contending parties, except perhaps on a very limited scale, and certainly without the strong connotations of praise and blame—and of official praise and blame, involving serious practical consequences—which are found in the U.S.S.R. Some of the labels, if attached at all, would probably have been attached the other way round; thus any speaker who supported one view on the ground that it emanated from British sources, or attacked the opposite view because it was foreign, would probably have been criticized for introducing nationalism into science.

* This apparently verbatim French translation frequently differs from the official English translation in ref. 3; presumably it is a summary.

In any event, participants in such a discussion in Britain would certainly not seek support for their views from authority, or from any particular philosophy or party, and it is inconceivable that the discussion should have ended in the laying down of an official line in science, with the corollary that certain scientific views should be officially combated. The most that could happen as the direct or indirect result of such a discussion would be that the extent of Government support for one line of research would be increased, for another diminished.

* * * * *

It remains to discuss the purely scientific aspects of the controversy; for although these, as I would like to reiterate, are subsidiary to the major issue, of the freedom and unity of science, they are of great importance.

Most important is what can only be termed the unscientific attitude of all the out-and-out Michurinites. In their work, they rarely employ scientific controls, statistical tests, or normal scientific precautions such as concern for purity of material, and they seem never to test the validity of alternative explanations; and their publication of methods and data is frequently quite inadequate to permit either repetition of the work or proper evaluation of the results claimed.

They do not carry on discussion in a scientific manner. I would have liked to substantiate this, too, by citations; but space forbids. My readers must accept my word for it. They neither demand nor accept the same kind of evidence as professional scientists elsewhere, they confuse facts with doctrine and theory with hypothesis or with belief, they misuse or re-define terms to suit themselves, they appeal to past authority instead of to present established fact and to utility instead of truth, they accept other than scientific criteria, or even insist upon them, in what purports to be a scientific argument. In brief, as Ashby and I found in conversations with Lysenko, they simply do not talk the same language as Western men of science. Lysenko himself, in the 1939 discussion, said, "if you want to get a particular result, you will get it".

In general, there is a lack of appreciation of the special character, validity and significance of the scientific method as developed in the last few hundred years. I commend the verbatim report to all those concerned for the future of science, although I fear they will find it very depressing reading.

Five main points call for comment. First, the repudiation of the theory of probability as applied in neo-Mendelian genetics and neo-Darwinian evolutionary theory. Secondly, the repudiation of the whole edifice of neo-Mendelian genetics itself. *Per contra*, and thirdly, the adducing, in support of Michurinism or Lysenkism, of results which *prima facie* appear more readily or solely explicable in terms of neo-Mendelism. Fourthly, Lysenko's claims to have induced an inheritance of acquired characters through treatments such as partial vernalization; fifthly, his claims to have altered the genetic constitution of organisms through grafting with other varieties or species.

As regards the last two points, they have been discussed by various general biologists, not specialists in these aspects of genetics; for example, at length by Hudson and Richens (1946), and more popularly by Waddington (1948–49). More relevant comments come from experts who have themselves worked on the same problems, such as Crane (1949), Darlington (1947), Ashby (1946), etc. Ashby had the rare opportunity of seeing Lysenko's experiments in progress.

I cannot go into details here; but the general upshot appears to be as follows. First, Lysenko's claim to have induced the inheritance of acquired characters by alterations in the conditions of life, notably the hereditary conversion of winter into spring cereals by partial vernalization applied for a few years, is quite certainly not proved and almost certainly unfounded.

To start with, there is an enormous volume of negative evidence from the work of biologists in many countries, showing that, when proper scientific precautions were exercised, no evidence can be obtained of any Lamarckian inheritance due to changed conditions.

Lysenko, unfortunately, did *not* exercise proper scientific precautions. As Waddington (1948–49) says, "the alleged hereditary changes are followed through too few generations, the genetical purity of the initial material is not adequately ensured, the possibility of selection is not always ruled out".

However, Waddington appears to me to have been over-cautious in this as well as in other judgments. I would say with assurance that selection was *never* ruled out, and must actually have been operative. Lysenko certainly has not taken the scientific precautions which in Great Britain would be regarded as elementary. Ashby saw his experimental plots, and tells me that their variability was deplorable; yet a high degree of genetic purity is essential in such experiments. Without genetic purity, selection will automatically operate between the different types, and will then simulate a Lamarckian effect. Furthermore, he never seems to have employed adequate controls, or indeed, in many cases, any controls at all.

As Darlington (1947) pithily writes: "The evidence as a whole shows that Lysenko is making use of the three classical precautions needed for the success of experiments designed to prove the inheritance of environmental effects: namely, beginning with a mixed stock, omitting to use proper controls, and repudiating statistical tests".

There is the further possibility, suggested to me by Ashby, that partially vernalized plants would fruit at a later, and therefore colder, date than fully vernalized plants (sown in spring) or normal untreated plants (sown in autumn), and that accordingly their seed might well be automatically partially vernalized in the ear. This again would simulate a Lamarckian effect, although it would actually be nothing more than a modification repeated in each generation.

The onus is on Lysenko and his followers to repeat the experiments, using adequate material and methods, and taking account of the various sources of error familiar to all competent geneticists.

As regards his claim that the characters of plants can be permanently altered by grafting, a few of his results may be factually correct, but most are either highly dubious or actually invalid, being due to errors in technique or to other causes than so-called vegetative hybridization, which is certainly an erroneous theoretical interpretation. As Ashby (1948) has pointed out, the mainstay of his (Lysenko's) evidence is the original experiments of Avakyan and Yastreb (published in 1941), which were discredited in 1944. In most of these, no precautions were taken

against cross-pollination in the so-called graft-hybrids.

Ashby and I ascertained in 1945 that the crucial experiments had been independently repeated in the U.S.S.R., using the same strains, but with adequate controls. Ashby later found that they showed conclusively that the strains used by Lysenko's followers were genetically far from pure, and that this fact, and not the grafting, accounted for many of the new forms which appeared: this would have been obvious if controls had been used in the original experiments. However, permission could not be obtained for the results to be published. Wilson and Wittmer (1946) in the United States repeated the experiments with various pure strains of tomatoes, and found no effects, genetic or otherwise, of stock on scion or vice versa. As Crane (1949) points out, many solanaceous plants are periclinal chimæras, and in some cases, as Jorgensen and Crane showed more than twenty years ago, following up the work of Winkler and of Baur, grafting may itself produce a chimæral structure. Such chimæras may then 'throw' variant forms, but by means which have nothing to do with "vegetative hybridization".

In passing, the results obtained in potatoes by Lysenko, where he claims that removal of eyes 'brought out modified heredity' (see Ashby, 1946, p. 263), undoubtedly depend on the chimæral nature of the stock. Ashby tells me that, after seeing the results, he considers that the new forms may have been produced from a virus-free layer.

It has been suggested that some of Lysenko's results may be due to the passage of some living (self-reproducing) substance between stock and scion. This may well be true. For example, as Salaman and Le Pelley discovered in 1930, pieces of King Edward potatoes induce a virus disease in other strains. The simplest explanation of this is that some form of protein normally present (and not deleterious) in King Edwards is what Darlington (1948) calls a pro-virus—that is, can multiply in alien tissues, causing harmful effects (Darlington, 1944). Even if some of Lysenko's results depend on a similar mechanism, this would not be a new discovery, nor would it have anything to do with the hybridization of two heredities by grafting, as Lysenko claims.

Finally, Crane (1949) directs attention to the fact that in his own very extensive (and painstaking) work on grafting, carried out on a large range of plants, no evidence of hereditary effects, let alone of vegetative hybridization, has ever been obtained.

There may be a few positive results of Lysenko's grafting work which are not due to faulty technique, to absence of scientific precautions, or to failure to understand the nature and behaviour of chimæras. These will need confirmation by others, and, above all, analysis in the light of the existing knowledge, not merely interpretation in terms of Michurin's and Lysenko's crude theories of hereditary constitutions being "shattered" (or "shaken"), or of their "assimilating" each other and the influences of external conditions—theories which do not link up with the main body of biological science.

In any event, they have no bearing whatever on the validity or otherwise of established neo-Mendelian facts and laws; but would merely represent an addition to existing knowledge.

This brings me to point two—the repudiation of neo-Mendelism, first by Lysenko and now officially by the Communist Party of the U.S.S.R. This is what men of science from other countries will find it hardest to follow—or to excuse. Throughout the discussion, Lysenko and his followers treat neo-Mendelism (or Morgano-Mendelism or whatever other title they apply to modern genetics) as a mere theory, in the sense of a hypothesis, not in the usual sense in which it is used in science, of a set of conceptions tying together a vast body of experimental results and established laws; what is more, they treat it as a theory inspired primarily by philosophical and political principles hostile to Marxism and Communism, and not by the desire to find the simplest explanation of the facts of Nature.

It cannot be too strongly reiterated that neo-Mendelian genetics is not just a theory in either of these two limited senses. Indeed, it can now rightly claim the title of genetics *tout court* as a branch of science in its own right (I may recall that what we now call simply bacteriology was for a time referred to as 'the germ theory'). It consists, first, of a really portentous accumulation of facts, some of observation but the majority experimental; then of the laws (such as those of segregation, independent assortment, linkage, etc.) which subsume many of the facts; of the mathematical calculations (often later verified by experiment), which permit a quantitative understanding of the action of selection on Mendelian populations; of the concepts and principles, such as those of material particulate inheritance (the theory of the gene), the principles of chromosome behaviour, and the principles of selection, which give the simplest general conceptual framework for interpreting the facts and laws; and finally of the interpretations, some wholly successful, others more speculative but at least coherent, of the processes of species-formation and long-range evolutionary change, in terms of neo-Mendelism and selection.

I repeat that modern genetics, with its neo-Mendelian basis, is now an autonomous branch of science, covering much of the field of evolution as well as of individual inheritance. Thus in repudiating "Morgano-Mendelism", the Michurinites and the Communist Party of the U.S.S.R. have repudiated not a mere speculative hypothesis nor a theory motivated by other than scientific reasons, but a large body of tested scientific fact, and a number of well-validated scientific laws.

Sir Henry Dale (1949), in his letter of resignation from the U.S.S.R. Academy of Sciences, brings this point out admirably. "The whole great fabric of exact knowledge, still growing at the hands of those who have followed Mendel, Bateson and Morgan, is to be repudiated and denounced."

Further, he makes clear what are the real issues, by describing the action of the Academy as one of the "attempts to suppress or to mutilate scientific truth in the interests of an extraneous creed", and by writing that "many of us . . . have been proud to think that there were no political frontiers or national varieties in a science common to all the world, but this is now to be separated from 'Soviet Science' and repudiated as 'bourgeois' and 'capitalistic' ".

Prof. H. J. Muller, probably the ablest and certainly the most all-round geneticist that the world has yet seen, has also resigned his corresponding membership.

The Academy, in its official replies, has descended to extraordinary depths for a scientific body. It calls both Dale and Muller "tools in the hand of reactionaries", and implies that they could have had to

expect persecution in their respective countries if they had not resigned. It states that Sir Henry Dale has shown himself a partisan "of the theories which were in vogue in Hitlerite Germany, which served as pretext for so many sanguinary horrors, and are still defended by the upholders of slavery and racial discrimination such as the Americans, who wish to impose their hegemony on the world". It adds that all believers in progress will approve the measures taken "and to be taken" by the Academy "to destroy for ever the continuance of this criminal obscurantism".

This last point again brings out the issue—that the U.S.S.R. is judging scientific fact and theories, not on the basis of their verifiable truth or falsity, but in relation to a particular social or political philosophy; and that in order to discredit those parts of science which they consider inimical to that philosophy, even the highest scientific body does not hesitate to employ violent abuse.

Coming down to details, Hitler's appeal to Mendelism in support of his racial policy was scientifically quite unjustified; for another, the fact of his having made such an appeal is entirely irrelevant to the truth or falsity of neo-Mendelian genetics as a branch of science; and finally, most neo-Mendelians are anti-racist. Then it is untrue that "the Americans" believe in slavery. It is not the Americans as a whole, or the United States as a nation, but only various separate States of the Union that uphold anti-Negro discrimination. Further, the statement that the United States ("the Americans") aim at world hegemony is really not very relevant (even if it were true) to Sir Henry's resignation.

Finally, the assertion that Sir Henry Dale's arguments "are not scientific in character, but are solely dictated by political motives and serve the anti-democratic and anti-Soviet campaign initiated in certain Western States" is an insult to common sense, as well as to Sir Henry personally and to the integrity of men of science in general.

(To be continued)

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RADIOACTIVE TRACERS

DURING the recent meeting of the Chemical Society held in Oxford, two sessions were devoted to discussions on the preparation and properties of radioactive tracers. On the afternoon of March 29, the papers were mainly concerned with the chemical aspects of tracer production, while on the morning of March 30 the bias was towards the nuclear physical side.

Five of the papers were devoted to descriptions of improved methods for the preparation of some of the more useful radio-isotopes or their compounds in a state of high specific activity. In circumstances where only a rather weak neutron source is available it is difficult to separate bromine-82 in high specific activity from irradiated sodium bromate. E. Berne (Göteborg) showed how this can be done by means of a two-stage process, using a non-isotopic carrier such as chlorine for the first stage, followed by a second stage using a small amount of inactive bromine in the usual way. The amount of non-isotopic carrier can thus be increased so as to give an efficient separation in the first stage without at the same time lowering the specific activity of the final product. A. W. Kenny and W. R. Maton (Harwell) described a method for the preparation of iron-59 of high specific activity from pile-irradiated hydroferrocyanic acid, involving extraction with ethereal hydrochloric acid. In this case, the product is contaminated with iron-55, from the natural iron-54, and if a radiochemically pure product is desired, the Co^{59} (fast n, γ)Fe^{59} reaction must be used. Methods for the separation of phosphorus-32, iodine-131 and sulphur-35 from pile-irradiated sulphur, tellurium and potassium chloride were also discussed by A. W. Kenny and W. T. Spragg (Harwell).

Preparations of radio-isotopes of high specific activity frequently form only the starting material for the synthesis of some particular compound, and where it is desired, as in certain biological experiments, that the specific activity of the compound should be as high as possible, special micro techniques may have

to be developed. Methods for the conversion of 10–20 micromoles of radiocarbon dioxide into acetylene, ethylene, ethyl iodide and ethyl magnesium iodide were described by W. J. Arrol and R. Glascock (Harwell and London). E. Glueckauf, R. B. Jacobi and G. P. Kitt (Harwell) have also developed a method for the production of carrier-free organic radio-halides. These are formed during neutron irradiation by interaction of the recoiling radio-halogen atom with an organic molecule (cf. Glueckauf and Fay, *J. Chem. Soc.*, 390; 1936) and can then be separated carrier-free, by a chromatographic technique.

The valency and state of combination of the atoms produced by neutron capture is a field which has been little explored. U. Croatto and A. G. Maddock (Cambridge) find, in agreement with previous workers, that the sulphur produced in the neutron irradiation of potassium chloride exists entirely in the hexavalent state. A study, by E. Broda (Vienna), of the yield in the Szillard–Chalmers reaction with the MnO_4^- ion using fast and slow neutrons showed that the yield is independent of the recoil energy over the range 150–10,000 eV. This suggests that the primary product undergoes the necessary chemical reactions subsequent to the loss in collisions of the kinetic energy derived from the nuclear reaction, and a mechanism was proposed involving the dissociative removal of an oxygen ion in the primary reaction yielding the MnO_3^+ cation.

All the above-mentioned papers dealt with β-emitting isotopes, the analytical determination of which depends on the use of β-ray counting techniques. L. Yaffe (Chalk River) described some interesting work on back-scattering of electrons in which the effect of varying the energy of the β-rays and the atomic number and mass thickness of the back scatterer was determined. Empirical relationships were given from which correction factors can be applied in any given case.

Recent work in American radiochemical laboratories associated with large cyclotrons was the subject of four notable contributions. J. W. Irvine, jun. (Massachusetts Institute of Technology), pointed out that the cyclotron is specially useful for the production of a number of valuable radio-isotopes. Deuteron reactions are favoured since the product can usually be separated carrier-free by a chemical method. Sodium-22 and -24, manganese-52 and -54, iron-55 and -59 and zinc-65 were quoted as examples. The calculation of yields is greatly facilitated if the deuteron excitation function for a particular reaction is determined. This is best done by means of the stacked foil technique (Clarke and Irvine, *Phys. Rev.*, 66, 231; 1944), and an example was given where twenty-four copper foils were stacked in the beam and afterwards dissolved simultaneously in twenty-four separate equipments, and radiochemical analyses for copper, zinc and nickel carried out. In this way, the excitation functions for the reactions Cu^{65} $(d, 2n)Zn^{65}$, Cu^{63} $(d, 2n)Zn^{63}$, Cu^{63} $(d, p)Cu^{64}$ and Cu^{65} $(d, 2p)Ni^{65}$ were obtained.

The production, separation and characterization of a number of neutron-deficient isotopes, using the 60-inch cyclotron at Berkeley, was described by G. Wilkinson (Berkeley). Separation was achieved by means of precipitation processes, or by ion exchange in the case of the rare earths. The decay of these neutron-deficient isotopes is complex, occurring by orbital electron capture, positive and negative electron emission and X-ray and γ-ray emission. They were characterized by using a simple β-ray spectrometer in conjunction with absorption measurements. 7·7-hour, 9·6-day and 85-day isotopes of thulium, prepared by the reactions $Ho^{165}(\alpha, 3n)$, $Ho^{165}(\alpha, 2n)$ and $Ho^{165}(\alpha, n)$, respectively, were described, and new isotopes of platinum, gold, tungsten, hafnium and neodymium have also been prepared.

The work of Prof. G. T. Seaborg's team at the University of California on the reactions produced by high-energy particles from the 184-inch cyclotron was reviewed by G. Wilkinson. The Bohr theory, which is applicable to the case of low-energy bombardments, does not account for the effects produced by high-energy protons and neutrons. Thus, according to the Bohr theory, bombardment with 100-MeV. protons might be expected to yield products ten to twelve mass units lighter than the target, whereas the product which is obtained in highest yield is actually within a few mass units of the target atom. The bombardment of copper with 190-MeV. deuterons was quoted as an example. Here the chief products occur at masses 60 and 62 (from Cu^{63} and Cu^{65}), although there is very little decrease in yield until 10–15 mass units have been lost where a second maximum is indicated at mass 51–53. There is a sharp drop in yield below this, and at 30 mass units lost it is only about 1/100 of that at 15 mass units lost. These results were interpreted in terms of Serber's theory (*Phys. Rev.*, **72**, 1114; 1947) of high-energy particle reactions. Under such conditions, the primary effect is attributed to an inelastic scattering of the bombarding particle by an individual nucleon, and where this occurs near the surface of the nucleus, only the struck nucleon and a few neighbours may be lost, thus accounting for the high yields at mass 60–62. In the case of a head-on collision, multiple collisions may occur in the interior of the nucleus, resulting in the dissipation of a much greater quantity of energy, and the high yield of particles with a loss of up to 15 mass units has been attributed to this process.

G. Wilkinson also described the recent work of Perlman, Ghiorso and Seaborg on the systematics of α-decay systems. It has been shown that when the α-energies are plotted against mass numbers in the case of elements from emanation ($Z = 86$) to curium ($Z = 96$), a series of parallel straight lines is obtained, one for the isotopes of each element. In the case of the lighter elements from bismuth ($Z = 83$) to emanation ($Z = 86$), a maximum appears with decrease of mass number, followed by a minimum, after which the curve resumes the same trend as shown by the heavier group of elements. Bismuth-207, polonium-210 and astatine-211, which lie close to the minimum of their respective curves, each contains 126 neutrons, and it has been suggested that this is probably a very stable configuration. Plots of α-energy versus logarithm of the half-life showed that nuclei with an even number of protons (Z) and an even number of neutrons ($A–Z$) fall on a series of nearly parallel curves with an average decrease in the probability of α-emission of about ten for an increase of 2 in Z. Nuclei containing an odd number of protons or neutrons or both tend to have a lower probability of α-emission (that is, longer half-lives). This can be accounted for to some extent by discontinuities in nuclear diameter and changes in nuclear spin, but it is suggested that it may be partly accounted for by an increased difficulty in assembling the constituents of the α-particle in nuclei containing an odd number of nucleons.

R. Spence

OBITUARIES

Mr. Will Hay

WILLIAM THOMPSON HAY, son of the late W. R. Hay, of Aberdeen, was born on December 6, 1888. As a lad, he was apprenticed as an engineer; but at the age of twenty-one left the workshop and went on the stage. Within ten years he had climbed to the top of the tree as a variety artiste and, as Will Hay, had become famous in the role of comic schoolmaster. During the next twenty years—the years between the two World Wars—he achieved great success in this character, and in the latter part of the period also took the leading role in many film comedies. In the summer of 1946 he had a serious illness from which he never completely recovered, and he died of a heart attack on April 18.

From an early period of his life, Hay had been keenly interested in astronomy, though he did not allow this to be generally known, either to his professional colleagues or to the public at large. He had, in fact, a dread of the use which might be made of such knowledge by certain sections of the Press, for he was deeply serious in his attachment to his chosen hobby and feared that it might come to be regarded as a sort of publicity 'stunt' to increase his popularity as a comedian. For the same reason he had hesitated to publish an elementary introduction to astronomy which he had written, and which later appeared under the title of "Through My Telescope", the name of the author being given as W. T. Hay.

On August 3, 1933, while observing Saturn with the 6-in. Cooke refractor mounted in his home-made observatory at Norbury, he detected a large white spot on the planet's equatorial zone. With a full realization of the rarity of such a phenomenon, he immediately made his discovery known to the astronomical world through the usual channels. The cat was now out of the bag, and Hay had the exasperating task of explaining to numerous reporters that even a comedian has a right to be serious in his private life.

In 1935 he moved to Hendon, where the few hours that he could spare from his professional work were divided between his observatory, where he made micrometrical measures of the positions of comets, and his workshop, where he made many useful pieces of accessory apparatus, both for himself and for his fellow amateur astronomers. A Blink microscope and several electrically driven chronographs were among the products of his mechanical skill, and he contributed papers on the construction of such apparatus to the British Astronomical Association, on the Council of which he served for a time. He had been a fellow of the Royal Astronomical Society since 1932, and made many friends among astronomers, both professional and amateur. He leaves a widow, a son and two daughters. W. H. STEAVENSON

Dr. J. A. Cushman

DR. JOSEPH AUGUSTINE CUSHMAN, who died at his home in Sharon, Massachusetts, on April 16, was one of the leading authorities on the Foraminifera. Born at Bridgewater, Massachusetts, in 1881, he was educated at Harvard University, where he obtained his doctorate of philosophy in 1909. During 1903–23 he was museum director of the Boston Society of Natural History. In 1923 he founded the now famous Cushman Laboratory for Foraminiferal Research, the first institute in the world devoted to the study of the Foraminifera. Dr. Cushman's drive and determination, and devotion to this natural order, were responsible for building up great collections of Foraminifera both fossil and recent from all over the world, consisting of more than 62,000 catalogued slides and an even greater number uncatalogued. Many type and figured specimens are incorporated, a great number of which have been described in the Laboratory's journal, *Contributions from the Cushman Laboratory for Foraminiferal Research*, which has been issued quarterly since 1925.

Dr. Cushman's text-book, "Foraminifera, their Classification and Economic Use", first published as a special publication of his Laboratory in 1928, has been issued in four editions, the last appearing only last year. This classic work soon became the standard text-book, invaluable both to the student and to petroleum companies who use Foraminifera as zone fossils in the stratigraphical correlation of cores. It was a new and revolutionary attempt in setting out genera in taxonomic and phylogenetic order, and is in itself an undying memorial to this great champion of Foraminifera. His total number of publications amounted to more than five hundred, some published alone and others jointly.

He was lecturer in micropalæontology at Harvard in 1926 and a consultant on Foraminifera to the United States Geological Survey since the same date. In 1930 he was appointed chairman to the Commission on Micropaleontology on the National Research Council.

Besides being a fellow of the American Academy of Arts and Sciences, of the American Geographical Society, and of the Geological Society of America, of which he was vice-president in 1938, he was a member of many societies, clubs and associations, including the Paleontological Society, of which he was president in 1937, and the American Association of Petroleum Geologists. He also became a member of the Society of Economic Paleontologists and Mineralogists, of which he was president for the year 1930–31, and edited the *Journal of Paleontology* during 1927–30. In 1937 the honorary degree of doctor of science was conferred upon him by Harvard University. In 1938 he was elected an honorary fellow of the Royal Microscopical Society in Great Britain.

Dr. Cushman has bequeathed his entire collections, library and catalogues to the Smithsonian Institution. They will eventually be transferred to the United States National Museum, where a room will be provided for what will be known as the Cushman Collection, and the work initiated by him on the Foraminifera will be continued there—a fitting memorial to this great student of the group.

C. D. OVEY

WE regret to announce the following deaths:

Dr. P. H. Cowell, F.R.S., formerly superintendent of the Nautical Almanac Office, on June 6, aged seventy-eight.

Prof. E. J. Garwood, F.R.S., emeritus professor of geology and mineralogy in the University of London, on June 12, aged eighty-five.

Prof. W. W. Hansen, professor of physics at Leland Stanford University, California, known for his work on the klystron and micro-wave electromagnetic radiation.

Mr. Philip Lake, formerly reader in geography in the University of Cambridge, on June 12, aged eighty-four.

NEWS and VIEWS

King's Birthday Honours

THE following names of men of science and others associated with scientific work appear in the King's Birthday honours list:

O.M.: Sir Robert Robinson, president of the Royal Society; Earl Russell.

C.H.: Mr. Lionel G. Curtis, president of the Royal Institute of International Affairs.

G.B.E.: Sir Walter Moberly, chairman of the University Grants Committee.

D.B.E.: Dr. Harriette Chick, for services to the study of nutrition.

Knights: Prof. David Brunt, professor of meteorology, Imperial College of Science and Technology, London; Dr. R. G. Hatton, lately director of the East Malling Research Station, and consultant director of the Commonwealth Bureau of Horticulture and Plantation Crops; Mr. H. S. Souttar, consulting surgeon, London Hospital; Prof. Godfrey H. Thomson, Bell professor of education, University of Edinburgh; Prof. J. A. Scott Watson, chief scientific and agricultural adviser, Ministry of Agriculture.

C.B.: Mr. H. Campion, director of the Central Statistical Office, Cabinet Office; Mr. W. G. A. Perring, director of the Royal Aircraft Establishment, Farnborough.

C.M.G.: Dr. F. Dixey, geological adviser to the Secretary of State for the Colonies, and director of Colonial Geological Surveys; Mr. H. Faulkner, deputy engineer-in-chief, General Post Office; Dr. C. H. Hampshire, secretary of the British Pharmacopœia Commission; Dr. D. E. Hansen, of Christchurch, New Zealand, for outstanding services in the field of technical education.

C.B.E.: G. H. Cunningham, for services in plant research and plant diseases; Dr. A. H. Davis, senior superintendent, Armament Research Department, Ministry of Supply; Mr. G. H. A. Field, director of research, Aluminium Laboratories, Ltd.; Mr. C. F. Franklin, consultant, Marconi's Wireless Telegraph Co., Ltd.; Prof. John Garstang, chairman of the British Institute of Archæology in Ankara; Dr. F. H. K. Green, assistant secretary, Medical Research Council; Dr. N. A. Mackintosh, director of research, Discovery Committee; Dr. J. R. Nicholls, deputy Government chemist; Prof. R. M. F. Picken, provost and Mansel Talbot professor of preventive medicine, Welsh National School of Medicine; Prof. A. D. Ross, professor of mathematics and physics, University of Western Australia; Mr. F. G. Simpson, archæologist; Dr. G. F. Herbert Smith, for services to the preservation of the flora and fauna of the British Isles; Dr. T. E. A. Stowell, for services to industrial medicine; Mr. R. G. White, director of the Animal Breeding and Genetics Research Organisation, Agricultural Research Council.

Geology at Cardiff: Prof. A. H. Cox

PROF. A. HUBERT COX, who has held the chair of geology in University College, Cardiff, since 1918, is to retire at the end of this session. A graduate of Birmingham and Strassburg, he had already published important papers on a wide range of subjects, and this extensive interest in research in both pure and applied geology has characterized the activities of the Department at Cardiff. Prof. Cox, alone or in collaboration, has made valuable contributions to our knowledge of the sequence and structure of the Cader Idris–Dolgelly region and of north Pembrokeshire. His interest in coalfield geology, shown first by his work on South Staffordshire fireclays, led to the recognition of the significance of the group of red beds (the Deri Beds) in the coal measures of Monmouthshire and East Glamorgan; recent extensions of this investigation, jointly with L. R. Moore, have led to a renewed interest in the sequence of coal measures in the east of the South Wales coalfield. Prof. Cox's early work on the factors controlling variations in the thickness of the Upper Carboniferous sediments in part of South Wales marked another important advance. His concern with coalfield problems led him to a study of mine dusts and their relation to 'silicosis', and to attempts to use electrical resistivity methods for the detection of water-filled areas in disused mines. As a recipient of the J. B. Tyrrell Fund from the Geological Society he spent several summers working in Canada. His influence has always been exerted in support of geology as an educational subject. Tribute was paid to his contributions to research by the award in 1948 of the Lyell Medal of the Geological Society.

Dr. J. G. C. Anderson

DR. J. G. C. ANDERSON, of the Geological Survey, who is to succeed Prof. A. H. Cox as professor of geology at University College, Cardiff, received his early training under Sir Edward Bailey at Glasgow. After graduating, he carried out postgraduate research, obtaining a Ph.D. in 1935. During his stay at Glasgow he was awarded the Mitchell Trust Bursary in science, and the Mackinnon Scholarship in geology. He was appointed to the Scottish Office of the Geological Survey in 1937, and worked for a time on routine work in the west of Scotland. When the outbreak of war interrupted normal Survey work, Dr. Anderson, in common with other members of the staff, undertook a series of investigations into the mineral resources of Scotland, the results of which were published in emergency wartime pamphlets. Among the materials investigated by Dr. Anderson were sands and gravels, silica rocks, limestone, oil shale and talc; and he was also the principal author of a Survey memoir on the Scottish granites. Apart from these official activities, Dr. Anderson has published a number of papers dealing especially with the igneous intrusions of the west of Scotland, and with the geology of the Dalradian rocks of the Highland Border region. Latterly he has extended his researches to the crystalline rocks of the west of Ireland, and has recently published an important paper on the Moinian rocks of Donegal.

Gold Medal of the Royal Astronomical Society: Prof. S. Chapman, F.R.S.

AT a meeting of the Royal Astronomical Society on February 11, the president, Prof. W. M. H. Greaves, presented the Gold Medal to Prof. Sydney Chapman for his contributions to geophysics and solar physics and particularly to the theory of geomagnetic phenomena (see *Observatory*, April). Prof. Greaves referred particularly to Prof. Chapman's work on solar and lunar daily magnetic variations for the years 1902 and 1905, which were sunspot maximum and minimum respectively, as a result of which he showed that the field responsible for the solar and lunar daily variation could be separated into a predominant part originating on the surface of the earth and a lesser part originating inside the earth.

He dealt with the researches by Prof. Chapman on magnetic storms, discussing especially the conclusions regarding the daily variations of forty magnetic storms—that the daily variations were additive to the normal daily variations—and to the deductions from these and certain other facts. Mention was also made of the theory of magnetic storms, formulated in 1931–32 by Chapman and Ferraro, which postulated a corpuscular stream emitted for a considerable time from a limited region of the solar surface. In this connexion, reference was made to Newton's work on solar flares, which does not support the view of Chapman and Ferraro on the comparatively long-lived streams. A note indicating the main lines of Prof. Chapman's work appeared in *Nature* of February 9, 1946, p. 155, when he was appointed Sedleian professor of natural philosophy at Oxford.

William Clift, F.R.S. (1775–1849)

One hundred years ago, on June 20, 1849, William Clift, one of the most lovable figures in John Hunter's world, died after more than half a century devoted to his master's cause. Clift was born in his father's flour mill at Burcome, near Bodmin, on February 14 (Hunter's birthday), 1775, and it was on his hero's birthday that, at the age of seventeen, he became Hunter's prosector and amanuensis. When a year later Hunter died, his executors, Matthew Baillie and Everard Home, retained Clift's services in the museum in Castle Street for seven years at a weekly wage of seven shillings. In 1800 the Government purchased Hunter's collections, entrusted them to the Royal College of Surgeons, and appointed Clift conservator. He was succeeded in 1842 by Richard Owen, who had married his daughter Caroline. Perhaps the most dramatic event in Clift's uneventful life was the evidence which he gave before the Parliamentary Committee on Medical Education in 1834 concerning the destruction of Hunter's manuscripts by Sir Everard Home. Fortunately, he had with loving care copied nine folios. His transcripts were published by Owen in 1861 under the title "Essays and Observations". A good anatomist and an accomplished artist, Clift enriched Matthew Baillie's "Morbid Anatomy" (1793) with fine copper-plate illustrations. Through the influence of Sir Humphry Davy he was elected to the Royal Society in 1823. Only five feet in height, timid, retiring and kind-hearted, with a "head crammed full of knowledge", Clift was a "meticulous gatherer of small facts", who was "always ready to impart and not to appropriate information".

The Fisheries School at Cronulla, New South Wales

In the *Australian Fisheries Newsletter* of February 1949, it is reported that after three successful courses for Commonwealth Reconstruction Training Scheme trainees at the Fisheries School, Cronulla, New South Wales (see *Nature*, **161**, 986; 1948), insufficient applications were received to warrant continuing the shore establishment. Only eight trainees have enrolled for a fourth course, and these will live and train on board the instructional vessel *A. R. McCulloch*. In addition to the *A. R. McCulloch*, a 25-ft. power boat and net boats are also available to the trainees, and instruction will be given again, as in previous courses, in both trawling and seining methods. Basic instruction in net and rope work will take place in the boat shed, and the whole course will last for seventeen weeks. Capt. H. Johnston, who had been technical superintendent of the School since its establishment in 1946, resigned at the end of last year to accept a post with Australian Fishing Industries, Ltd. His successor is Mr. A. O'Grady, himself a former trainee of the school, having passed through its first full course.

Even though it be at the other side of the world, the experience of the Fisheries School at Cronulla provides valuable lessons for similar interests in Great Britain. It is felt in some quarters that one or more schools of fishing should be established here. It is clear that much careful thought will be needed to make a success of such projects and to ensure for them more than a brief and temporary existence. The need for such training in Britain is obvious; but the machinery by which it can be provided will be hard to devise within the framework of the existing industry.

Derivation of the Names 'Quartz' and 'Gneiss'

Until recently, the origin of the familiar name 'quartz' was a complete mystery. In 1942 (*Min. Mag.*, **26**, 172), Dr. S. I. Tomkeieff proposed an ingenious explanation which he now summarizes in a more recent article (*Geol. Rundschau*, **36**, 98; 1948) containing a stimulating discussion of the derivations of certain old rock-names which have long resisted etymological research. Four hundred years ago, Agricola, to whom the name 'quartz' or 'quertz' was known, stated that the small veins that cross the larger metalliferous veins were called *Querklüfte* by the Saxon miners. At that time the word *Ertz* was applied to gangue minerals, including spars, as well as to ores. Tomkeieff makes the plausible assumption that the quartz of the cross-veins of Saxony was called *Querklüftertz*, which in course of time became condensed to 'queretz' and 'quertz' and finally to 'quartz'. Support for this explanation comes from Cornwall, where the mineral was formerly known as 'cross-course spar', a name suggesting a direct translation from *Querklüftertz*. 'Gneiss' or 'kneiss', also known to Agricola, has been a familiar rock-name since the days of Werner, though its origin has remained unknown. Tomkeieff learned from an old Czech miner in Joachimsthal that the country rock in which the metalliferous veins occur—the 'nest' of the ores—was called *hnisto*, a Czech word meaning 'nest'. The corresponding word in Russian is *gnezdo*, and in the other Slavonic languages it is similar. The suggestion that here we have the source of the name 'gneiss' is one that carries conviction.

Army Education

A thought-provoking article by Prof. T. E. Jessop on religion in army education appears in the current issue of the *Journal of the Royal Army Educational Corps* (**23**, No. 1; March 1949). Starting from his experience that many serving soldiers have little moral or spiritual background to guide them in their everyday lives, Prof. Jessop suggests that, while the direct teaching of Christianity should remain the function of the Royal Army Chaplain's Department, much indirect teaching could be carried out by the Royal Army Educational Corps. In the remainder of his article he suggests ways whereby this could be done through current affairs, history, literature and science. Other articles in the *Journal* deal with the state of education in the British Army of the Rhine, the backward adult, Ashridge as a college for citizenship and the present work of the Institute of Army Education, Eltham Palace, and the Army School of Education, Bodmin.

Sickness in the Population

THE Registrar-General has recently issued the second of the monographs dealing with medical and population subjects (Sickness in the Population of England and Wales in 1944–47; H.M. Stationery Office. 1*s*.) For more than a century the statistics of the General Register Office have provided a basis for attacks by medical science on the killing diseases; but it is only in recent years that an attempt has been made on a national scale to measure ill-health which does not necessarily cause death, but which leads to great economic loss as well as individual and social disturbance. Before ill-health can be measured it must be defined, and for any published morbidity statistics to have real meaning the terms used should command general acceptance. In the present stage of development of this subject terminology is neither precise nor uniform, and here the author of the monograph, Dr. Percy Stocks, chief medical statistician to the General Register Office, makes an attempt to standardize terminology by describing in detail the terms he uses in the interpretation of his material. He then examines the value and limitations, as indices of morbidity, of information derived from notifications of infectious diseases, from the Survey of Sickness, carried out monthly since 1943, and from statistics of special food allowances given for certain diseases.

Training the Employment Interviewer

AN article on the means of making employment interviewers more efficient in selecting and allocating applicants for jobs has been written by John Munro Fraser in the current issue of *Psychology at Work* (2, No. 2, May 1949). These courses have been organised by the National Institute of Industrial Psychology and are arranged in groups of three, the first of the group having been started in September 1946 to provide instruction in the systematic approach to selection and allocation and to give supervised practice in interviewing. On the second course, which was arranged some months later, students are given more intensive practice in interviewing. A little later the third course takes place, and here the personnel officers attending are shown how to use and interpret a series of intelligence tests suitable for different levels of employees. Already a number of these grouped courses have been arranged in different parts of Great Britain, and the results have been entirely satisfactory, a reasonable standard of competence having been reached by the students.

Fourth Empire Mining and Metallurgical Congress

A THIRD circular on the coming Fourth Empire Mining and Metallurgical Congress has recently been published. Details of this Congress have already been reported (see *Nature*, February 5, p. 206), but this new circular, besides covering all the points previously mentioned, contains the final programme details in the various centres, a list of papers to be read and a list of delegates. The general time-table is: July 9–12, inauguration of the Congress in London, and short excursions in the London area; July 13–17, technical sessions in Oxford; July 18–23, excursions to a choice of one of the four following centres—Cardiff, Cornwall, Edinburgh or Newcastle-on-Tyne. The sessions in Oxford will be organised as follows: *A* and *B*, mineral resources; *C*, modern methods of prospecting; *D*, physiological and psychological effects of heat and humidity on workers in deep mines and metallurgical works; *E*, petroleum; *F*, coal; *G*, present-day trends in mineral dressing; *H* and *I*, metallurgy and metallurgical industries. In addition to the technical papers which will be presented at these sessions, three evening lectures will be delivered: "Metallurgical and Mining Aspects of Atomic Energy" by Sir John Cockcroft, director of research (atomic energy), Ministry of Supply; "Miners as Pioneers" by Dr. Charles Camsell, formerly Deputy Minister of Mines and Resources, Ottawa; and "The General Theory of Metals and Alloys" by Dr. W. Hume-Rothery, lecturer in metallurgical chemistry, University of Oxford. Besides the member States of the British Commonwealth, eleven other countries will be represented at the Congress, as follows: Belgium, Chile, Egypt, Holland, Iraq, Italy, Norway, Spain, Sweden, Switzerland and the United States. Further information may be obtained from the Joint General Secretaries, Fourth Empire Mining and Metallurgical Congress, 436 Salisbury House, Finsbury Circus, London, E.C.2.

Summer School in Theoretical Physics at Bristol

THE H. H. Wills Physical Laboratory and the Department of Adult Education of the University of Bristol, in co-operation with the Institute of Physics, will be conducting in Bristol during September 19–24 a summer school in theoretical physics, with particular application to crystal growth and related problems. Lectures will be given by Prof. N. F. Mott, Dr. F. C. Frank and others. The present course is similar in conception to those of previous years, and is intended mainly for research students at universities and for the members of the staffs of Government and industrial laboratories who wish to familiarize themselves with the theoretical aspects of subjects with which they are, or have been, concerned on the experimental side. In addition to the regular lectures, there will be opportunities for research workers attending this course to discuss their own work and its theoretical implications. The course will refer to the development of ideas brought forward at the Conference of the Faraday Society in April of this year. The fee for the course is six guineas, but a limited number of free places will be available for students. The total number attending the course will be limited to about fifty. Further particulars and forms of application, to be returned before July 10, can be obtained either from the Director of the Department of Adult Education, University, Bristol, 8, or from the Secretary of the Institute of Physics, 47 Belgrave Square, London, S.W.1.

Annual Meeting of the Society of Chemical Industry

THE annual general meeting of the Society of Chemical Industry will be held during July 11–15 at Whitworth Hall, University of Manchester, under the presidency of Sir David Rivett. Special features of the meeting will be the presentation on July 13 of the Medal of the Society to Dr. Foster D. Snell, of New York, who will give an address on "Detergents and Detergency", and on July 14 the Ivan Levinstein Memorial Lecture by Prof. Paul Karrer, of Zurich, on "A Short Survey of the Biological Significance of the Vitamins and Related Substances". The programme of the meeting will consist mainly of lectures, works visits and social excursions. The British Rayon Federation is arranging an exhibition of fabrics at Ashburne Hall during the week of the meeting. Further information may be

obtained from the honorary secretary of the Manchester Section of the Society, Dr. W. H. Brindley, c/o Hardman and Holden, Ltd., Manox House, Miles Platting, Manchester 10.

Fourteenth International Veterinary Congress

THE Fourteenth International Veterinary Congress will be held in London during August 8–13, and the meetings will take place in the Central Hall, Westminster, and in Church House, Westminster (see *Nature*, February 5, p. 205). It is now known that H.R.H. The Duke of Gloucester will be unable to open the Congress. The main theme of the Congress is the veterinary profession's contribution to world food supplies, and at the plenary meetings in the Central Hall the first address will be given by Lord Boyd Orr on "The World Food Situation". At the sectional meetings, papers (printed and circulated in advance) will be presented by delegates, from many parts of the world, who will discuss animal health and disease control problems. Further information on the International Congress may be obtained from the Organising Secretary, Fourteenth International Veterinary Congress, 10 Red Lion Square, London, W.C.1.

The National Veterinary Medical Association will this year confine its annual congress activities to three days, two of which will coincide with meetings of the International Congress, so that there may be joint discussions; the third day will be devoted largely to demonstrations at the Royal Veterinary College, Camden Town, London, N.W.1. The Association congress will afford opportunities for international discussion on small-animal matters, as opposed to the farm livestock subjects which will occupy the time of delegates during the first four days.

Traditional Art of the British Colonies

AN exhibition of "Traditional Art of the British Colonies" will be opened at the Royal Anthropological Institute, 21 Bedford Square, London, W.C.1, on June 21, and will remain open throughout the "Colonial Month", of which it is one of the principal features, and will be open between the hours of 10 a.m. and 7 p.m. on Mondays to Saturdays, closing on Wednesday, July 20. This will be an unusual opportunity for studying the ancient cultures of Nigeria, as well as masks and figures from other African Colonies, from Malaysia and from Oceania. A full descriptive and illustrated catalogue (price 1*s.* 6*d.* to visitors) underlines the relevance of the scientific data of anthropology to æsthetic appreciation.

Special Science Exhibition for the Blind

THE first exhibition in Great Britain solely for blind persons is being held at the Science Museum, South Kensington, London, S.W.7, during June 11–26. It has been organised by the Science Museum in conjunction with the National Institute for the Blind, and suitable exhibits covering a wide range of popular science are available for examination by blind visitors. The exhibition includes models and apparatus relating, for example, to the many forms of transport (railways, shipping and aircraft), to mining, the textile industries, hand and machine tools, time measurement, meteorology, printing and typewriting. Descriptive labels, transcribed into Braille, explain the exhibits, and printed labels are available for escorts to read to those not proficient in Braille. As the Exhibition is intended solely for the blind and partially sighted, it is not open to normally sighted members of the public unless acting as escorts to blind visitors. The Exhibition is free and is open daily from 10 a.m. until 6 p.m. (Sundays 2.30–6 p.m.).

Lady Tata Memorial Fund Awards

LADY TATA MEMORIAL FUND international awards for research in blood diseases, with special reference to leukæmia, have been awarded for the year 1949–50 to the following, for work at the places mentioned. *Grants* for research expenses and assistance: Dr. M. C. Bessis (Paris); Dr. J. Bichel (Aarhus); Dr. P. Cazal (Montpellier); Dr. J. Clemmesen (Copenhagen); Dr. E. Kelemen (Szeged); Dr. J. Chandra Mehta (Agra); Prof. C. Oberling (Paris); Dr. Edith Paterson (Manchester); Dr. G. Schallock (Münster); Prof. E. Storti (Pavia). *Scholarships*: Dr. P. Atanasiu, of Rumania (Paris); Dr. C. F. M. Plum (Copenhagen).

University of London: Appointments

THE following appointments in the University of London have been announced: Mr. R. S. Edwards, to the University chair of economics with special reference to industrial organisation tenable at the London School of Economics and Political Science as from October 1; Dr. G. A. D. Haslewood, to the University chair of biochemistry tenable at Guy's Hospital Medical School as from October 1. The title of reader in mathematics in the University has been conferred on Dr. Daniel Pedoe, in respect of the post held by him at Westfield College. The following doctorates have been conferred: *D.Sc.*: H. J. S King (Northern Polytechnic), F. O. Howitt (West Ham Municipal College) and B. C. Saunders (an external student).

Announcements

MR. T. DALLING, chief veterinary officer, Ministry of Agriculture and Fisheries, has been elected president of the Royal College of Veterinary Surgeons for the year 1949–50.

A SYMPOSIUM on "Photochemistry in Relation to Textiles" is being arranged by the Society of Dyers and Colourists and will be held at Harrogate during September 22–24. At this symposium, which is open to non-members, special emphasis will be laid on the scientific and technical aspects of the fading of dyed materials and the photodegradation of both dyed and undyed materials. Further information may be obtained from the Assistant Secretary, Society of Dyers and Colourists, 32–34 Piccadilly, Bradford, Yorks.

THE Department of Radiology of the University of California Medical School will be holding a post-graduate course on physics in radiation therapy during September 6–9, after the course on the medical aspects of nuclear energy during August 29–September 3 (see *Nature*, May 14, p. 760). The course will be conducted under the chairmanship of Dr. Robert S. Stone, professor of radiology, and ten lectures will be delivered by Dr. Edith H. Quimby, associate professor of radiology, Columbia University. Ionizing radiations from various types of sources will be considered. Further information may be obtained from Prof. Stacy R. Mettier, Medical Extension, University of California Medical Center, San Francisco 22, California.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

Uptake of Radioactive Phosphorus by Influenza Virus

To gain information about the mechanism of virus synthesis in the host cell, we have been carrying out experiments on influenza virus growing in the embryonated egg. It was of particular interest to determine whether virus growing in the presence of radioactive phosphorus would incorporate the isotope into its structure. Some of the results are described here.

As a preliminary to the study with phosphorus-32, it was necessary to determine the distribution of phosphorus in the virus. Purified virus for this purpose was obtained as follows. Eleven-day embryonated eggs were inoculated with 10–100 ID_{50} of influenza virus A (*PR* 8 strain) by the allantoic route. After 48 hours incubation at 36° C. the allantoic fluid was harvested and the virus concentrated and purified by adsorption on, and elution from, red blood cells, followed by two cycles of differential centrifugalization. Several preparations were examined with the electron microscope; they contained little electron-absorbing material apart from the elementary bodies and a few of the rod-like forms typical of this virus. In several such preparations, one ID_{50} for embryonated eggs contained 5×10^{-14} to 4×10^{-15} gm. nitrogen.

Extraction of dried purified preparations with a mixture of alcohol and ether (3/1) removed about 25 per cent of the total weight and 50 per cent of the total phosphorus of the virus. Most of this alcohol–ether soluble material was also soluble in petroleum ether and, in accordance with previous observations[1], was assumed to contain all the virus phospholipid. The phosphorus remaining in the alcohol–ether insoluble fraction constituted about 0·5 per cent of the virus weight: the total phosphorus content of the virus was about 1 per cent.

Previous workers[2,3] have observed that at least part of the phosphorus of the alcohol–ether insoluble residue is distributed between pentosenucleic acid and desoxypentosenucleic acid. In an attempt to separate these two acids quantitatively, the procedure of Schmidt and Thannhauser[4] was applied to the residue; about 94 per cent of the phosphorus separated with the pentosenucleic acid fraction.

In experiments with radioactive phosphorus, phosphorus-32 as sodium phosphate was inoculated into the allantoic cavity of embryonated eggs at an arbitrary interval three hours after injection of virus. In one of our experiments the radioactive solution inoculated into each egg registered 57,000 counts per minute on a Geiger–Müller counter. The allantoic fluid was harvested after about forty-eight hours, and the virus, purified by the procedure described above, had a specific activity of 6·6 counts/min./μgm. phosphorus. The phospholipid fraction registered 3·0 counts/min./μgm. phosphorus, while the alcohol–ether insoluble residue gave 10 counts/min./μgm. phosphorus. An attempt has been made to ascertain the specific activities of the pentose- and desoxypentosenucleic acid fractions separated from the alcohol–ether insoluble residue by the method of Schmidt and Thannhauser. By far the greater proportion of the phosphorus-32 was associated with the pentosenucleic acid fraction. Sufficient material has not yet been available at any one time to make a satisfactory estimate of the specific activity of the desoxypentosenucleic acid fraction.

Strong supporting evidence that the isotope was, in fact, incorporated into the virus structure was afforded by control experiments which demonstrated that there was little or no direct physical exchange *in vitro* of phosphorus-32 between radioactive sodium phosphate and the virus; nor did the radioactive virus exchange its phosphorus-32 when dialysed against phosphate buffer.

This work has been aided by a grant from the National Cancer Institute of Canada. We wish to thank Dr. C. E. van Rooyen for his interest throughout. The work is being continued, and a full report will appear later elsewhere.

A. F. Graham
Laurella McClelland

Connaught Medical Research Laboratories,
University of Toronto.
Feb. 4.

[1] Taylor, A. R., *J. Biol. Chem.*, **153**, 675 (1944).
[2] Knight, C. A., *J. Exp. Med.*, **85**, 99 (1947).
[3] Beard, J. W., *J. Immunol.*, **58**, 49 (1948).
[4] Schmidt, G., and Thannhauser, S. J., *J. Biol. Chem.*, **161**, 83 (1945).

Reactivation of Ultra-Violet-Inactivated Bacteriophage by Visible Light

Coli-bacteriophage of the 'T' group[1] are inactivated at a logarithmic rate by ultra-violet light. Some of these phages can be reactivated inside bacteria that adsorb more than one inactive phage particle[2]. I recently observed another type of reactivation of ultra-violet irradiated phage, namely, reactivation upon exposure to visible light in the presence of bacterial cells (photo-reactivation). Since this phenomenon may cause serious misinterpretations of results obtained in working with irradiated phage, it may be useful to report it at this early stage of its investigation.

The occurrence of photo-reactivation of ultra-violet irradiated phage was noticed accidentally a few weeks after receiving a personal communication from Dr. A. Kelner that he had discovered recovery of ultra-violet treated spores of Actinomycetes upon exposure to visible light. I am informed by Dr. Kelner that his results are in course of publication[3]. My observation indicates the correctness of Dr. Kelner's suggestion that the phenomenon discovered by him may be of general occurrence for a number of biological objects.

Photo-reactivation of bacteriophage is most easily observed in the following conditions. Phage is exposed to ultra-violet light (2537 A.) in a transparent medium. Equal diluted samples of the irradiated phage are plated with a large excess of sensitive cells on a series of nutrient agar plates by the agar-layer technique[4]. Some of the plates are incubated in darkness, others in strong light; the latter after incubation show a considerably larger number of plaques (more than a thousand times under optimum conditions). The extent of photo-reactivation depends, among other things, on the dose of ultra-violet light, the intensity and composition of visible light, and the time of exposure to it. All seven phages of the 'T' group give photo-reactivation, although in

different amounts, the extent of photo-reactivation being apparently specific for each phage type. The most effective wave-lengths in the visible range appear to be the shorter ones; particularly effective is the 4047 A. line of the mercury arc. Under our experimental conditions, no measurable reactivation was produced by wave-lengths longer than 4400 A.

Photo-reactivation is not only observed upon exposure to visible light on agar, but also when mixtures of irradiated phage and bacteria in a liquid medium are exposed to visible light before plating. No reactivation is observed, however, when either the irradiated phage or the bacterial cells, or both, are exposed separately to visible light before mixing. The relation of photo-reactivation to the various phases of host-range relationship is not yet clear. There are indications that the role of bacteria in photo-reactivation may be a complex one.

The conditions of illumination under which photo-reactivation occurs are such that no appreciable amount of reactivation takes place in the conditions employed in experiments on reactivation by multiple infection[3], although no special precaution had until recently been taken to eliminate visible light of short wave-length in the course of such experiments. Recent experiments on reactivation by multiple infection carried out in weak yellow light gave the same numerical results as the earlier ones.

No photo-reactivation has been observed with phage inactivated by X-rays.

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R. Dulbecco

Department of Bacteriology,
Indiana University,
Bloomington, Indiana.
Jan. 22.

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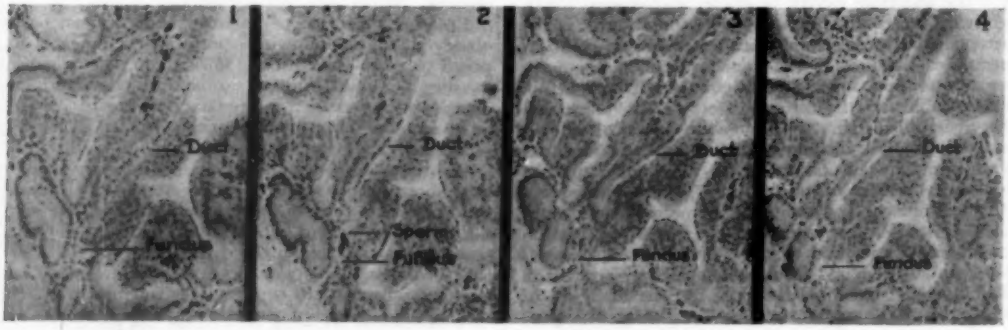
Structure of the Sperm-nests in the Oviduct of the Domestic Hen

O. Charnock Bradley[1] reported the existence of short ducts in the grooves of the infundibular mucosa of the hen's oviduct. Walton and Wetham[2] mentioned the probability that a structure for the storage of sperm existed in the genitalia of the hen. It was considered unlikely that the life-duration of spermatozoa could be retained for the three to five weeks reported by Barfurth[3], Payne[4], Crew[5] and others, without some form of concentration in the organs somewhere near the site of fertilization. The view of Ivanov[6] regarding fertilization of immature ova was not considered acceptable, and was, in fact, not fully supported by their data.

With the aid of a new technique, intraperitoneal insemination[7], it was found possible to obtain a larger number of chicks from one insemination than had been recorded previously. It was also possible to demonstrate morphologically unaltered spermatozoa in the oviduct of the hen twelve days after insemination by applying a new method involving capillary force[8]. This led to the discovery of the 'sperm-nests' in histological sections of the infundibular mucosa of fertile hens eight and eleven days after insemination[9].

Careful examination of these structures in a number of preparations using different staining techniques has now convinced me that the short ducts of Bradley are identical structures to the 'sperm-nests' reported recently. In connexion with the earlier histological observations mentioned[1], it may be added that the non-ciliated cells at the fundus of the ducts are in my opinion not true goblet cells, although the nucleus is situated close to the basal membrane, as in the goblet cells found among the ciliated epithelium in the ducts and on the folds. It is suggested that the fundus cells form the prototype epithelial cells of the mucosa at the main points of cell division. This may be partly responsible for the formation of the invaginations in the grooves resulting in the short ducts. These cells should thus be looked upon as young epithelial cells, and it is not inconceivable that they may perform a special function in the maintenance of the viability of sperm stored in the sperm-nests. The spermatozoa assume a position with their nuclei or heads closely packed against the fundus cells. Their position reminds one of the arrangement of unripe sperm cells in the testis. Thionin-stained sections of the ducts containing spermatozoa show no mucin secretion in the cells of the fundus; but the walls of the ducts contain numerous cells filled with blobs of mucin, showing them to be actively secreting goblet cells.

Further histological investigations are contemplated, but an idea of the structure of the sperm-nests may be obtained from the accompanying four photographs of serial longitudinal sections (5 μ thick) of a sperm-nest. The material was obtained from a hen killed eight days after intraperitoneal insemination. Live motile spermatozoa were demonstrated in the infundibulum of this hen during a laparotomy



operation under nembutal anæsthesia immediately prior to slaughter.

Drs. C. Jackson, M. de Lange and H. P. A. de Boom, of this Laboratory, generously advised and assisted with the histological preparations. I am indebted to Mr. Theo. Meyer for the photomicrographs, and to the Director of Veterinary Services for permission to publish this report.

G. C. v. Drimmelen

Veterinary Research Laboratory,
Onderstepoort.
Dec. 14.

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Asymmetry of the Reproductive Organs in the Earthworm *Neodrilus agilis*

Neodrilus agilis n.sp. is the second species of the genus, and is distinguished from *N. monocystis*, Beddard, 1887, by the possession of two pairs of calciferous glands, situated in segments XIV and XV, and by many small diverticula of the spermatheca clustered around the spermathecal duct.

In a specimen of *N. agilis* collected from Karori, Wellington, on June 30, 1948, there is asymmetry of the reproductive organs. There is no reference to such asymmetry in the literature available to me.

Normally *Neodrilus* has a single pair of spermathecal pores, in the intersegmental groove 7/8, each pore situated anterior to the chætæ *ab*. In this specimen the left spermathecal pore is present in the normal position; but the normal site of the right pore is covered by a white scar, 0·5 mm. in width, which extends across the ventro-lateral surface of the body, from the middle of segment VII to the posterior border of segment VIII. Chæta *b* is absent on both VII and VIII, on the right side.

The right prostatic pore is present on XVII, the left missing; but the ventral chætæ are absent on the left side, which is the condition when a prostatic pore is present.

Internally, the left spermatheca has the form usually found in the species, that is, a large thin-walled sac in VIII, opening to the exterior at 7/8 by a wide, openly convoluted duct, and having a number of small diverticula clustered around the distal extremity of the duct. The right spermatheca is absent, and the right nephridium of VIII is divided into two small pieces. The left prostate is absent, although the right prostate is present and has the form normal in the species, that is, a convoluted tubular structure arising from a slender muscular duct in XVII, and passing through the segments XVII–XIX, with a sac containing penial chætæ in association with its duct in XVII.

Apart from the irregularities of the spermathecæ and the prostates, the internal anatomy is normal.

It seems likely that the absence of the right spermatheca is due to injury; but the absence of the left prostate cannot be explained in this way since there is no external or internal scar, and it is unlikely that such a large structure as a prostate, extending as it does through several segments, could be completely excised without considerably distorting the internal anatomy in the region of the prostates. There is not likely to be any connexion between the excision of the right spermatheca and the failure of the left prostate to develop, since I have found in a large number of specimens of the species which I have dissected that the spermathecæ and the prostates are always at similar stages of development, and it would appear that they develop contemporaneously. This presents all the evidence of a case of asymmetry of the reproductive organs due to some accident in the development of the specimen.

K. E. Lee

Department of Zoology,
Victoria University College,
Wellington, New Zealand.
Nov. 2.

Southern Beech (*Nothofagus*) Flowering Seasons

The periodic occurrence of 'mast' years followed by prolific regeneration of the European beech (*Fagus sylvatica* L.) has enabled the European forester to 'domesticate' this species to such good effect that he has produced with it some of the finest managed forests known. A similar seeding behaviour in the closely related southern beeches (*Nothofagus* spp.) of New Zealand is of interest because of the growing economic importance of our southern beech forests. Sawn timber from them forms a small portion of the annual cut, while timber used in the round is of considerable importance.

Although there are a number of records of the gregarious regeneration to be seen in New Zealand beech forests, there have hitherto been only passing references to the periodic heavy flowering and seeding years. An opportunity was presented of studying one of these in the spring–summer season of 1948–49, when an exceptionally heavy flowering occurred in most districts in four species, *N. fusca* (Hook. f.) Oerst., *N. truncata* (Col.) Cockayne, *N. solandri* (Hook. f.) Oerst. and *N. cliffortioides* (Hook. f.) Oerst., but only in a few places in the fifth species, *N. menziesii* (Hook. f.) Oerst. The flowering has been followed in general by an equally heavy seed set.

An indication of the coming flowering was obtained while examining dormant buds in the winter prior to spring flowering. Flower primordia were then discovered in large numbers in the buds. A certain amount of precocious flowering, mainly of staminate flowers, also occurred in the previous late summer and autumn. Thus it was realized that the factors bringing about a heavy flowering are in operation during the summer prior to flowering. It is believed by European foresters[1] that beech masts follow hot seasons. German work[2] has shown that before mast years the European beech accumulated heavy reserves of starch. Summer drought, usually occurring in hot seasons, then altered the carbon–nitrogen ratio, which in turn initiated flower primordia[3].

New Zealand meteorological records show that the 1948 flowering was preceded by a hotter summer than average. These hot seasons occur, usually throughout most of New Zealand, at irregular intervals. They are not necessarily accompanied by drought. In addition to the 1948–49 season, it has been possible, from the scanty records available of heavy flowering and seeding seasons, to correlate two of them, 1938–39 and 1935–36, and a possible

third in 1907–8, with previous hot summers. The gap between 1908 and 1935 is simply a gap in records, for several seasons undoubtedly occurred in this interval.

The behaviour of *N. menziesii* needs to be examined further, for it does not appear to fall into line with that of the remaining species. In other characters, morphological, physical properties of the wood, etc., it is also distinct from its New Zealand relatives, and along with certain Australian and South American species should probably be placed in a separate section of *Nothofagus*.

It is of interest to note that specimens of *Fagus sylvatica* grown ornamentally also flowered heavily in the spring of 1948. Specimens of two South American *Nothofagus*, *N. obliqua* and *N. procera*, grown from seed brought to New Zealand some fifteen years ago, also flowered for the first time.

A. L. Poole

Botany Division,
Department of Scientific and Industrial Research,
Wellington, New Zealand.

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Tubular Factors in the Development of Extra-renal Azotæmia

In *Nature* of March 5, Gomori, Balint and Harsing[1] presented the results of an investigation into urea excretion under conditions of dehydration in the rabbit and demonstrated the significant fall in the ratio urea clearance to glomerular filtration-rate.

Glomerular filtration-rate (ml./min.)	Renal plasma flow (ml./min.)	Filtration fraction	Blood urea (mgm./100 ml.)	Urea clearance (ml./min.)	Urea clearance / Glom. fil.-rate
Normal					
108	520	20·7	31	50	0·463
142	640	22·0	28	62	0·437
122	580	16·0	22	54	0·443
Mean		19·5			0·448
After dehydration					
80	308	26·0	70	16	0·200
90	400	22·5	58	24	0·267
87	390	22·3	42	15	0·172
Mean		23·6			0·213

In a series of experiments, to be reported in full elsewhere, on man, similar findings have been obtained. The subjects were dehydrated by the simple process of water deprivation for a period of four days. The results presented in the accompanying table show clearly that (1) glomerular filtration-rate falls, (2) the renal plasma flow falls to an even greater extent, and consequently (3) the filtration fraction rises. That the fall in the ratio urea clearance to glomerular filtration-rate should be associated with an increase in the filtration fraction is of interest in view of the demonstration of Barclay, Cooke and Kenney[2] that with a fall in the filtration fraction the ratio urea clearance to filtration-rate rises to a value which in some cases may exceed unity. These two findings would suggest that there is available, in addition to the filtrative and reabsorptive mechanisms, an active tubular secretory process for urea, and that the balance of the two tubular components is related to the hæmodynamic state of the organ. It is then possible that the change observed in urea excretion in dehydration may have its origin in the hæmodynamic disturbance which occurs under these conditions.

R. A. Kenney

Department of Physiology,
School of Medicine,
Leeds 2.

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Phosphorus Compounds and Metabolic Rate in Insect Pupæ

The level of inorganic phosphorus varies in the blood of hibernating pupæ of the hawk-moth (*Celerio euphorbiæ*) inversely with the rate of consumption of oxygen[1,2]. The curve representing inorganic phosphorus values in the blood during the whole pupal stage is the reverse of the well-known U-shaped curve of oxygen consumption.

The 7-min. labile phosphorus (in adenosine triphosphate) in the pupal muscle diminishes during the winter diapause, when the metabolic-rate is at its minimum[3]. In 1941 we found[4] in the 'brei' of overwintering *C. euphorbiæ* pupæ a reduced cozymase-level, when compared with that of fresh pupa. Cozymase proved to be a limiting factor for the oxygen consumption in the brei. Added cozymase raised the respiratory rate of the preparation, but it was soon inactivated.

X-Ray photogram of *C. euphorbiæ* during pupal diapausis showing the solidified phosphate-rod

We have now made a further observation concerning the phosphorus metabolism during the diapause. In this period the intestine is quite shrunken, representing only 1–2 per cent by weight of the whole body. Its content is converted into a solid rod consisting of inorganic phosphate, probably potassium phosphate. A very important proportion of free phosphate is thus immobilized (see table).

The intestine contains 50–70 per cent of all inorganic phosphorus of the pupa. When calculated (as is usual) in mgm. per cent, it reaches values of

Exp. No.	Inorganic phosphorus (mgm.) in the whole body	in the intestine	Per cent in intestine
1	1·53	1·10	72
2	2·60	1·50	58
3	2·06	1·33	65
4	1·95	1·00	51
5	2·71	1·38	51
6	2·00	1·14	57

3–6,000 mgm. per cent, which has not been recorded before, so far as I know.

During the development period the inorganic phosphorus in the intestine diminishes. The remainder is found in the meconium excreted by the hatching adult[5].

The elimination from tissues of inorganic phosphorus and its deposition in solid form in the intestine seem likely to enhance the splitting action of phosphorylases in the tissue. This idea is in line with my earlier hypothesis[3] concerning the mechanism regulating metabolic rate during the pupal stage. The descending part of the U-shaped curve is thought to be conditioned by a phosphorolytical inactivation of coenzymes. The elimination of free phosphate enhances this process until an equilibrium is reached at minimum metabolic rate. During the development period the phosphate deposited is gradually dissolved and penetrates the tissues, reversing the trend toward synthesis.

JÓZEF HELLER

Laboratory of Animal Physiology,
Zoological Institute,
University of Wrocław.
Dec. 21.

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Early 'Criollo' Cacao in Surinam and the Origin of 'Forasteros' of Trinidad and Venezuela

THE history of so-called 'Criollo' cacao is interesting for the study of most cultivated 'Forasteros' hybrids.

There are two South American Criollos, namely, the 'Venezuelan' with purplish to red shell (before maturity), and another with pale green ('white') shell. The first iconography of the latter was published by Marie Sybille Merian (Frankfurt, 1647–Amsterdam, 1717[1]), daughter of the famous Swiss engraver Matthieu and sister of Matthieu the Younger. In this rare, beautiful book, one coloured plate shows, with quite fantastic leaves and young fruits (and a butterfly not associated with the cacao), flowers (inserted on young branches) and two ripe pods, one of which has fresh seeds in tangential section. Both pods are ripe, yellowish in colour, with tuberculate, deeply furrowed shell, long and narrow point, with shape intermediate between 'Cundeamor' and 'Angoleta'. The ratio of length to diameter of pods is 1·7–1·8. Seeds appear plump, with cotyledons pale reddish-violet or lilac in colour ('vinaceous-lilac', 69''' RV–V,*b*, according to Ridgway's standard[2]); or, in our colour classification, intermediate between Nos. 5 and 6[3].

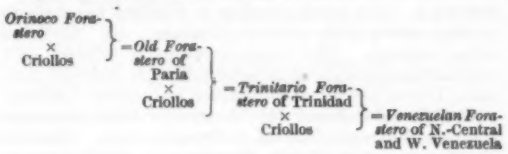
This form of var. *typica* Cif.[4] of *Theobroma cacao* L. (= *T. cacao* L. p.p. according to Pittier[5]), in our opinion, is identical with the "Criollo de la montaña", which has diffused from the higher Amazon basin to the northern side of South America, as far as Central America and probably Trinidad and Tobago, through the lower Orinoco basin[7].

The conclusion is that, so early as the beginning of the eighteenth century, Criollo was the only (or, at least, the most frequent) cacao cultivated in Surinam, as in Venezuela (and Colombia).

It is a matter of history[6] that, in the year 1727, a 'blast' (the nature of which is in dispute) virtually destroyed the Criollo cacao plantations of Trinidad; another and hardier kind of cacao being introduced, it has been supposed[6], from eastern Venezuela[7,8]. But it is also well established that, in Venezuela, until the year 1825, only the two Criollos were cultivated, with the possible exception of the Paria Peninsula (on the north-east side of Venezuela, near Trinidad), where we found a very mixed cacao population, quite distinct either from the Forastero of Trinidad and the derived Venezuelan Forastero[3], in spite of the possible later re-introduction of both Forasteros in the same region.

That the hardier cacao introduced into Trinidad after 1727 came from the Atlantic side of South America, from Orinoco to the Guianas, seems now improbable, since the most easily accessible, extensively cultivated strains would be used. Sources of possible introduction of cacao in Trinidad are then: (1) the Paria Peninsula of Venezuela, the ancient cacao population of which may be derived from local Criollos, hybridized with early introduced strains of the imperfectly known wild cacao of the Lower Orinoco (a sub-population of Amazonian Forastero); or (2) another West Indian island, such as Hispaniola (Dominican Republic and Haiti[9]).

Accepting the first hypothesis, the genealogy of Forasteros of Trinidad and Venezuela (excluding the Andean and Maracaibo basin populations of cacao) may be summarized as follows:



(the last one with many sub-populations, until we have the 'almost pure' Criollos of the north-west and west of Venezuela).

Accepting the second alternative, the supposed genealogy of both Forasteros is still more complicated, for it is related with the early introduction into Hispaniola of cacao seeds from the eastern and western sides of South America, of at least two Forasteros (a kind of Nacional of Ecuador and an Amelonado of the Guianas–Amazon region), superimposed on and/or hybridized with both Criollos, and with the possible early introduction of cacao strains from Central America in a bewildering mixture[10,11,12].

R. CIFERRI

Italian Cryptogamic Laboratory,
Botanical Institute,
University of Pavia.
Jan. 10.

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Fluorimetric Determination of 'Antrycide'

During trials of the new trypanocidal drug 'Antrycide' (Curd and Davey[1]) it became necessary to develop a method of determining it in plasma. This brief description of the method is published in the hope that it may be useful to other workers who use the drug.

'Antrycide' is colourless, and, although it shows blue fluorescence in ultra-violet light, a method based on this property was not sufficiently sensitive to measure the low concentrations usually found in plasma. Consequently, potential colorimetric methods were examined, particularly the general methods for organic bases that involve extraction of a coloured salt of the base into a solvent, such as those introduced by Prudhomme[2] and Auerbach[3], and developed mainly by Brodie[4,5]. Although salts of 'Antrycide' with all the thirty acid dyes examined were almost insoluble in ethylene dichloride and benzene, the solvents commonly used in procedures of this type, a technique developed by my colleague, Dr. J. Raventós[6], for the colorimetric determination of *d*-tubocurarine chloride, involving extraction of an eosin salt of the alkaloid into a mixture of chloroform and butanol, was successfully applied to 'Antrycide', and found to give a highly fluorescent extract. A summary of the method follows. So far as I am aware, a fluorimetric procedure of this type has not hitherto been described.

Special reagents. (1) Stock solution of eosin (500 mgm.) in saturated sodium bicarbonate (A.R., 500 ml.). This stock solution is purified by shaking it four times with solvent mixture. (2) Buffered eosin reagent. The stock solution is diluted 1/25 with saturated sodium bicarbonate. (3) Solvent mixture. Dilute 200 ml. of re-distilled butanol to 1,000 ml. with B.P. chloroform. (4) 'Antrycide' stock standard solution, 100 mgm./100 ml. (referred to ion). Dissolve 134·5 mgm. of dichloride dihydrate in distilled water and dilute to 100 ml. (5) 'Antrycide' working standard, 0·05 mgm./100 ml. Dilute the stock standard when required.

Special apparatus. Coleman electronic photofluorimeter, model 12*A*.

Procedure. Dilute 1·2 ml. of plasma to 10 ml. with distilled water. Add 2 ml. of 15 per cent trichloroacetic acid, mix well and allow to stand at least 10 minutes. Centrifuge, and transfer 10 ml. of the clear upper layer to a 60-ml. glass-stoppered bottle. Add 1 ml. of *N*-sodium hydroxide, 2 ml. of buffered eosin reagent and 12 ml. of solvent mixture. Shake vigorously for 3 minutes, and allow to stand until the lower layer has separated. Remove the latter, and clarify it by filtering through Whatman No. 1 or similar semi-fine paper into a Coleman fluorimeter cuvette. Read the fluorescence against a blank and standards in the Coleman fluorimeter. These are prepared by adding 0, 0·1, 0·3, 0·6, 1·0, 2·0 and 3·0 ml. of the 'Antrycide' working standard to 30-ml. bottles, treating with 2 ml. of buffered eosin reagent and 12 ml. of solvent mixture, and continuing the estimation as described above. Coleman *PC* 9 (pale yellow) visual filter and Coleman *B* 1 (low sensitivity) or *B*1*S* (high sensitivity) ultra-violet filter are used in the fluorimeter.

The procedure measures down to 50 μgm. of 'Antrycide'/litre of plasma with satisfactory accuracy (± 10 per cent) and down to 20 μgm./litre with fair accuracy (± 25 per cent). Under carefully controlled conditions, concentrations from 5 to 20 μgm./l. can be detected as 'traces'. 'Antrycide' added to normal plasma is not completely recovered. The loss of about 20–25 per cent is due to precipitation of 'Antrycide' with protein, and is increased if larger volumes of plasma are used. The method has a considerable degree of specificity, since tertiary bases appear not to react under the conditions described, and many quaternary bases (for example, *d*-tubocurarine chloride, 'Dimidium' bromide and 4 : 6-diaminoquinaldinemethochloride) give only weakly fluorescing extracts. However, it is not yet possible to state that the method does not measure metabolites of the drug.

A full description of this and three other, less sensitive, methods that have been developed will be published elsewhere.

A. Spinks

Research Laboratories,
Imperial Chemical Industries, Ltd.,
Hexagon House, Manchester 9.

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Modified Technique for the Development of Paper Chromatograms

In the original publication by Consden, Gordon and Martin[1] on the application of partition chromatography on filter paper to the separation of amino-acids, the positions of these acids were determined by spraying the whole of the paper with a 0·1 or 0·05 per cent solution of ninhydrin in butanol. This procedure appears to have been adopted by subsequent workers in spite of inherent objections to the use of butanol in a fine spray in more or less close proximity to the operator. To avoid this objection, and to determine whether this stage could be entirely eliminated, a series of 'runs' has been made in which the ninhydrin has been incorporated in the moving phase.

Ninhydrin is readily soluble in the usual solvents such as aqueous phenol, collidine or butanol–acetic acid, and a concentration of 0·1 per cent does not appreciably affect the R_F values of any of the amino-acids examined. The solution in phenol produces considerable intensification of colour with certain amino-acids, notably arginine and histidine, and this colour develops in a few minutes even at room temperature. The spots obtained are, however, abnormally elongated, which makes the use of phenol undesirable for this technique. With collidine or butanol–acetic acid as solvent this effect does not appear, and normal spots are produced.

The technique suggested for two-dimensional chromatography is to run initially in phenol as usual, and then at right angles using a 0·1 per cent solution of ninhydrin in collidine or butanol–acetic acid. On completion of the run, the paper is warmed in a current of air to remove the solvent, and this warming is sufficient to develop the colours of the individual amino-acids. The colours are more blue than those obtained after a ninhydrin–butanol spray, and are usually of greater intensity, so that smaller quantities may be identified.

D. E. Nicholson

School of Medicine,
Leeds 2. Jan. 28.

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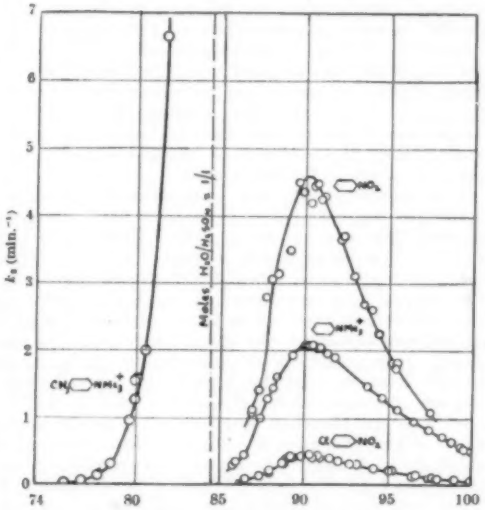
Nitration in Sulphuric Acid

In the nitration of 2 : 4-dinitrotoluene in homogeneous solution in 87–100 per cent sulphuric acid media[1], the effective nitrating agent is the nitronium (NO_2^+) ion[2]. The velocity coefficient k_2 of the equation

$$d[ArNO_2]/dt = k_2 [ArH] [HNO_3]$$

(which is valid for fixed initial conditions only) has a maximum (first discovered by Martinsen[3]) in 92·0 per cent sulphuric acid at 90°.

We have now extended the experiments to the nitration of (*a*) nitrobenzene, (*b*) *p*-chloronitrobenzene, and (*c*) the trimethylphenylammonium ion (introduced as its nitrate), all at 25°. The diagram shows the variations of k_2 with medium composition.



Composition of medium (% H_2SO_4)

Experimentally, the results show (i) that, as nearly as can be judged, all three substances are nitrated most rapidly, at 25°, in the same optimum medium of approximately 90·4 per cent sulphuric acid; (ii) that relative rates of nitration for these compounds do not vary greatly with medium composition[10].

Medium, % H_2SO_4	97·0	96·0	94·0	92·0	90·0	89·0	87·0
$k_2(a)/k_2(b)$	9·1	9·3	10·3	10·4	10·2	10·6	9·4
$k_2(c)/k_2(b)$	6·5	6·1	5·7	5·0	4·8	5·2	5·6

These kinetic features are consistent with the previous supposition[1] that, in the equation

$$NO_2^+ + ArH + B = ArNO_2 + BH^+,$$

an acceptor molecule B, furnished by the medium, plays a part in determining the rate of nitration in a highly acid environment; but doubt is thrown on the validity of this hypothesis by Melander's demonstration[11] that a tritium atom and a protium atom are replaced at the same rate in the nitration of toluene to dinitrotoluene.

The trimethyl-*p*-tolylammonium ion, which stands structurally to the trimethylphenylammonium ion as *p*-nitrotoluene does to nitrobenzene, is rapidly nitrated (see diagram) in media in which the molar ratio H_2O/H_2SO_4 exceeds unity. The same conclusion follows, for benzene[7] and toluene[8], from a comparison of their two-phase nitrations with those of nitrobenzene[5] and dinitrotoluene[6]. Chemical and physical evidence (especially Raman spectra[4]) shows that no substantial concentrations of nitronium ion can exist in such media. The nitrating agent may be the $H_2NO_3^+$ ion[9], or, perhaps more probably, the NO_2^+ ion, present in very small concentration.

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T. G. Bonner
Margaret E. James
Ailsa M. Lowen
Gwyn Williams

Royal Holloway College
(University of London),
Englefield Green,
Surrey.

Nov. 25. (Revised, May 10.)

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Separation of 2 : 4 Dinitrophenyl Amino-acids on Buffered Silica Gel

For some time past I have been making a study of the free amino groups of the peptides liberated by papain and sodium bisulphite acting on wool keratin[1], using Sanger's methods[2,3]. At an early stage it was found that 'non-absorbent gels' which were of service in separating N-acetyl amino-acids were of little use in separating 2 : 4 dinitrophenylamino-acids, which all ran together as a single fast band using chloroform as solvent. Phillips and Stephen[4] and Consden, Gordon, Martin and Synge[5] have previously commented on the difficulty of preparing suitable gels with reproducible properties. Sanger[2] and Consden *et al.*[3] considered that silica gels which permitted satisfactory separations 'absorbed' the dinitrophenyl amino-acids to some extent. I have observed that successful separations of these acids on silica gel can be achieved by using concentrated phosphate buffers instead of water as the stationary phase; by varying the *p*H of the buffer, the rate of movement of the band of a particular acid can be varied within wide limits, the higher the *p*H the slower being the rate.

In general, with a given solvent the most successful separations are brought about by using the most alkaline buffers, although the rates of band movement may then become very slow and 'tailing' be pronounced. In practice, a compromise is made between completeness of separation and speed at which the

band moves down the column, and a buffer of suitable *p*H is found by trial. On a given buffered column, no significant variation in rate of band movement of a particular dinitrophenyl amino-acid between different batches of gel is observed, and the difficulty involved in preparing a suitable gel does not arise. Using a single solvent in conjunction with columns buffered to different *p*H values, the number of such acids which can be separated from each other is considerably greater than the number which can be separated in the same solvent with unbuffered columns.

The method of separation of mixtures of dinitrophenyl amino-acids I have used, which is based on these observations, differs somewhat from that given by Porter and Sanger[3]. Buffered chloroform and butanol–chloroform columns are employed for the initial separations. Subsequent separations are carried out using buffered columns and other solvents. Buffered 5 per cent propanol*cyclo*hexane columns are useful for separating dinitrophenyl monoamino-acids from each other, and in general they bring about those separations that Sanger's ethanol–ligroin columns do. Thus on these columns dinitrophenyl–leucine can be separated from dinitrophenyl–valine and the latter from dinitrophenyl–alanine. The slower-moving dinitrophenyl amino-acids have characteristic rates on buffered ether columns and can be separated from each other in this way. The faster-moving acids have characteristic rates on buffered 33 per cent ether–ligroin columns, and it is probable that useful separations could also be brought about in this solvent.

The increasing use of buffered silica gel in partition chromatograms, for example, in the separation of fatty acids[4] and penicillins[5], suggests that this type of column may be capable of very wide application. A fuller description of the present experiments will be given elsewhere. I am indebted to the Director of the Wool Industries Research Association for permission to publish this note.

S. BLACKBURN

Wool Industries Research Association,
Torridon, Headingley,
Leeds 6.
Dec. 22.

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A Possible Nuclear Effect in Magnetic Cooling Experiments on Copper Salts

ATTENTION has recently been directed by Gorter[1] to the existence of a splitting of the energy-levels of paramagnetic salts due to the coupling between the electronic and nuclear magnetic moments. The level separation, which is of the order of 0·1 cm.$^{-1}$, is seen in the hyperfine structure of the optical line spectrum. Now such a splitting ought also to lead to a Schottky specific heat anomaly at temperatures of the order of 0·1° K., that is, some 10^4 times higher than in salts the paramagnetism of which is due only to the nuclei.

Of substances used in magnetic cooling experiments, it appears that the copper salts are almost unique in that copper possesses both electronic paramagnetism and a non-zero nuclear moment. The hyperfine structure of the optical arc spectrum of copper has been observed by Ritschl[2]. If the electric crystalline field and the other interactions in the crystal be ignored, then Ritschl's value of 0·022 cm.$^{-1}$ (0·031° K.) for the splitting-factor of the $^2d^9\ ^2D_{5/2}$ state (the ground-state in the crystal before the introduction of the crystalline field[3]) would lead to a specific heat of the order of $(0{\cdot}031)^2\ R/T^2 \sim 10^{-3}\ R/T^2$ per gm.-ion. Very recently a hyperfine structure has been observed by Penrose[4] in the paramagnetic resonance lines in very dilute copper salts, from which it appears that the splitting in the crystal is similar in order of magnitude to that observed by Ritschl for the vapour, so that the above estimate of the size of the specific heat should not be seriously in error. From a study of this resonance line pattern it should be possible to obtain a better estimate of the magnitude of the nuclear specific heat; but meanwhile it may be of interest to see what experimental evidence there is for its existence.

The specific heat and susceptibility of copper potassium sulphate below 1° K. have been measured by de Klerk[5] and by me[6]; I find that $C = 6{\cdot}1 \times 10^{-4}\ R/T^2$, a value which is some five times larger than one would expect from dipole–dipole interaction. The excess has been ascribed by Opechowski[7] to anisotropic exchange; but his theory gives no indication of the magnitude of the effect expected from this cause. It is possible that at least part of the excess specific heat may be due instead to the electronic-nuclear coupling under discussion, and it will be seen that the observed specific heat is in agreement as to order of magnitude with this idea. For copper sulphate, Ashmead[8] has found two maxima in the gamma-ray specific heat C^*, and it is probable that there are two corresponding maxima in the true specific heat. The upper maximum at about 1° K. can be understood as corresponding to the removal of the electronic degeneracy by exchange effects[9], but it has always been difficult to understand how both anomalies could be due to the electronic system. According to the present suggestion, the lower maximum might be caused by electronic-nuclear coupling, provided that this is not itself quenched by exchange effects.

Note added in proof.—Since this letter was written, Prof. Gorter has informed me that specific heat measurements on diluted copper potassium sulphate have been made by De Vrijer and theoretical calculations carried out by Broer, which, in general, confirm the views expressed in this letter.

C. G. B. GARRETT

Royal Society Mond Laboratory,
Cambridge.
March 26.

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High Indices of Refraction of Barium Titanate and other Heteropolar Titanium Compounds

It is well known that all compounds containing titanium ions surrounded by six oxygen ions show high indices of refraction, and with the aid of the Lorentz – Lorenz formula high values of the electronic polarizabilities are calculated. Unfortunately, this formula is not rigorously valid for any of these titanium compounds because of the low symmetry of the crystal structures.

For cubic barium titanate, however, a corrected Lorentz – Lorenz formula has been derived[1] in which the quantities P and Q are calculated to be $15{\cdot}1_8\ a^{-3}$ and $4{\cdot}3\ a^{-3}$ respectively (where a is lattice constant); and it can be shown that the deviations from the Lorentz – Lorenz formula are smaller than 0·5 per cent. From the index of refraction measured by Busch, Flury and Merz[2] (2·42 at 140° C.) we find $9{\cdot}44 \times 10^{-24}$ cm.^{3} for the total electronic polarizability of a $BaTiO_3$ 'molecule'. Considering that the variations of the electronic polarizabilities (α's) of positive ions in different lattices are small compared with those of oxygen ions, we can calculate an approximate value for α for oxygen ions, α_{OX}, by subtracting from the total electronic polarizability the contributions of Ba^{2+} and Ti^{4+} as given, for example, by Van Vleck[3] (1·56 and $0{\cdot}18 \times 10^{-24}$ cm.^{3}, respectively). Thus we find $\alpha_{OX} \approx 2{\cdot}6 \times 10^{-24}$ cm.^{3}, which is rather large compared with oxygen polarizabilities obtained for other heteropolar compounds: for MgO and α-Al_2O_3, we calculated $\alpha_{OX} \approx 1{\cdot}7$ and $1{\cdot}4 \times 10^{-24}$ cm.^{3}, respectively.

A tentative explanation of this phenomenon might be given in the following way. According to the dispersion theory, we can write:

$$\frac{n^2 - 1}{n^2 + 2} = \frac{4\pi}{3} N\alpha(\nu) = \sum_i \frac{c_i}{\nu_i^2 - \nu^2}, \qquad (1)$$

where n is index of refraction, N is number of $BaTiO_3$ 'molecules' per cm.^{3}, ν is frequency of light used for the determination of n, c_i is a measure for the oscillator strength of the characteristic frequency ν_i.

Considering that the electronic polarizability is mainly due to one type of ions (namely, the oxygen ions) we replace the sum in (1) by a single term

$$\frac{4\pi}{3} N\alpha(\nu) \approx \frac{4\pi}{3} N_{OX}\, \alpha_{OX}\, (\nu) \approx \frac{c}{\nu_e^2 - \nu^2}. \qquad (2)$$

From this the electronic polarizability is found to be

$$\alpha_{OX} = \frac{3c}{4\pi N_{OX}\, \nu_e^2}. \qquad (3)$$

A high value of α_{OX} may be due to a large value of c or a low value of ν_e. It appears from dispersion data, however, that for a large number of such heteropolar oxides where the electronic polarizability results practically from the oxygen ions alone, the ratio $c/N_{OX}\nu_e$ is approximately constant[4]. Thus in order to find a high value of α_{OX}, a low value of ν_e is sufficient.

The question now arises what processes are responsible for the polarizability. Elsewhere[4] we have suggested that one might assume that in these processes an electron is transferred from a negative ion to a cation, the latter being situated at such a distance that the interaction between the electron and the empty place left behind can be ignored. We can write:

$$h\nu_e \approx A_O \frac{e^2}{a} + (-A_M) \frac{e^2}{a} - I_M + E_O - \varphi_O - \varphi_M. \qquad (4)$$

where A_O, A_M are the Madelung constants for oxygen and metal positions; I_M is the nth ionization energy of the n-valent ion to which the electron is transferred; E_O is the second electron affinity of oxygen; a is the edge of the unit cell; φ_O, φ_M are the polarization terms due to the fact that both the electron and the hole left behind polarize the lattice. For cubic $BaTiO_3$, we calculated by Born's method[5]:

$$A_O = 6{\cdot}456, \quad -A_{Ti} = 12{\cdot}378, \quad -A_{Ba} = 5{\cdot}387.$$

For φ we used as a rough approximation the energy of a sphere with the same radius r as the ion and with charge e, embedded in a continuum with dielectric constant n^2.

$$\varphi = \frac{e^2}{2r}\left(1 - \frac{1}{n^2}\right).$$

Putting E_O equal to $-8{\cdot}9$ eV. we find:

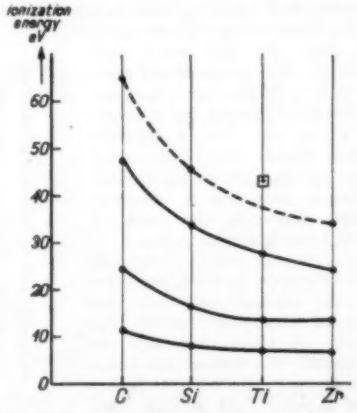
$h\nu$ (O → Ti) = 2·0 eV.; $h\nu$ (O → Ba) = 40·2 eV.

Thus the O → Ba transition can be expected to give only a small contribution to α_{OX} compared with O → Ti. A similar calculation gives for MgO: $h\nu$ (O → Mg) = 14·4 eV.; and for α-Al_2O_3, $h\nu$ (O → Al) = 14·4 eV. This provides a qualitative explanation of the difference between α_{OX} of $BaTiO_3$ and α_{OX} of MgO or α-Al_2O_3.

It should be noted that in these considerations more details of the crystal structure are involved than in those of Fajans and Joos[6], particularly since the Madelung potentials at the positions of the electron both before and after the 'jump' and the ionization energy of the metal enter into equation 4.

By the same method as applied above, it is possible to give a qualitative explanation of the fact that all compounds containing titanium ions surrounded by six oxygen ions show high refractive indices. If we compare the fourth ionization energy of titanium, which plays a part in (4), with that of other tetravalent metals, it appears to be about 7 eV. larger than would be expected when intrapolating smoothly between Si and Zr (see graph). Thus, all other factors being the same, the values of ν_e are unusually low for titanium compounds.

Some further experimental facts can be connected with this high fourth ionization energy: (a) the



First four ionization energies for some tetravalent elements

occurrence of compounds with trivalent titanium, whereas trivalent silicon and zirconium are unknown; (*b*) the weak temperature-independent paramagnetism shown by TiO_2 [7]. If the titanium were present in the form of ions with rare-gas configurations, only diamagnetism would be observed. Thus we may suppose that there are excited states situated at such a small distance above the ground-level as to give rise to an appreciable temperature-independent paramagnetism.

Finally, it is to be remarked that this explanation of the high electronic polarizability of the TiO_4 configuration is different from that given by Shockley [8], who supposes it to be caused by a large value of c (equat. 3), resulting from the possibility of many transitions (for example, to *s*-, *p*- or *d*-orbits), rather than from a small distance between these levels and the ground-level.

J. H. VAN SANTEN
F. DE BOER

Philips' Research Laboratories,
N. V. Philips' Gloeilampenfabrieken,
Eindhoven.
Dec. 3.

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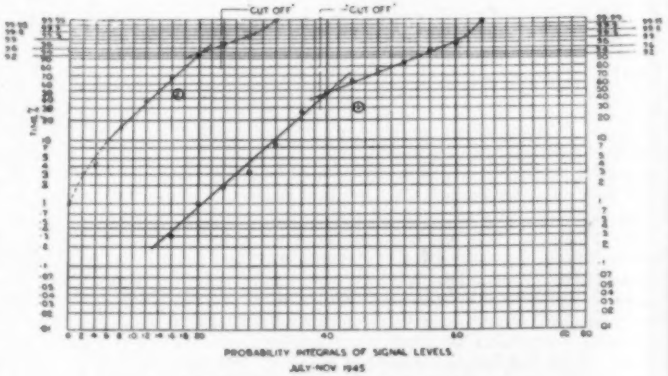
Atmospheric Effects on Short-Wave Radio Propagation

In a recent note[1], we showed that for oversea paths which were low and non-optical, the received radio field-strength on centimetre wave-lengths was much increased by the presence of low-level atmospheric ducts lying directly on the sea. These ducts, which trap the radiation, appear to have heights of up to about 100 ft. over the sea around the coasts of Britain. The purpose of this note is to show that, whereas such ducts have a pronounced effect on the propagation of centimetre-wave radiation for paths lying predominantly within the ducts, they have little effect on the same wave-lengths when either or both transmitting and receiving aerials are well above the top of the duct, or on metre wave-lengths even for the same low paths which show pronounced duct effects on centimetre wave-lengths.

The results obtained on a wave-length of 9 cm. for a 60-mile path, for which the heights of transmitting and receiving aerials were 100 ft. and 860 ft. respectively, are shown in the form of probability integrals in curve 1 of the accompanying graph. Rayleigh probability paper is used, and the results are plotted so that the ordinate of any point gives the percentage of time that the received signal was below the field-strength given by the corresponding value of abscissa. It will be seen that the experimental points lie along two different curves, the lower part of which approximates closely to a straight line at 45° to the axes of co-ordinates. Such a straight line on this paper corresponds to the Rayleigh distribution $y=2x\exp(-x^2)$. One phenomenon which would give such a distribution would be the field-strength at a receiving point which was the resultant of an infinite number of waves of infinitely small amplitude having random phase relation. The diffraction of waves around an obstacle corresponds to this condition, providing there is, for example, variation with time in the refractive index of the medium surrounding the obstacle and its magnitude is sufficient to introduce random effects along different paths. Because the experimental points appear to have this Rayleigh distribution, we would suggest that the lower portion of the curve corresponds to conditions in which the field at the receiver is the result of diffraction around the earth's curvature in an atmosphere of which the refractive index at any point varies randomly with time. The upper portion of the curve giving the higher values of field-strength might be assumed to represent conditions when atmospheric refraction was sufficient for the transmitting and receiving aerials to become intervisible, and for the field at the receiver to be no longer dependent on diffraction but the resultant of two rays, one direct from the transmitting aerial and the other after reflexion at the earth's surface. This hypothesis is supported by the fact that the intersection between the two portions of the curves in the graph occurs at a value of field-strength which agrees closely with the value (shown as 'cut-off') calculated on the assumption that the refraction of the atmosphere was just sufficient to bring the transmitting and receiving aerials within 'visible' range of one another.

Similar results on a wave-length of 3·5 m. for the same path, but with aerial heights each of only 100 ft., are shown in the second curve of the graph. These results are seen to be closely similar to those obtained on the shorter wave-length for a path much above the top of the expected ducts. It will be noted that, again, the intersection between the two portions of this second curve occurs at a value of field-strength agreeing well with the 'cut-off' value. When a similar analysis was made of the low-level centimetre links, for which the signal strength is controlled largely by the presence or absence of ducts, the probability integral curves were quite different in shape, being much more horizontal. The conclusion is, therefore, that for the links giving probability integrals of field-



(1) 9-cm. band high link; (2) 3·5-m. band low link

strength having the characteristic shapes shown in the graph the effects of low-level ducts are unimportant, and that it should be possible to calculate the field-strengths from normal refraction theory assuming a suitable refractive index gradient in the atmosphere.

During the period of the radio observations, measurements of temperature, humidity and pressure were made at regular intervals by the R.A.F. at a series of heights up to 1,500 ft. along the path. The results were used to determine the gradient of refractive index over various ranges of height. Using these values of index gradient, the signal-strength at the receiver was calculated. For the low-level metre wave-length link and the higher link on 9-cm. wave-length, the resulting field-strengths plotted as probability integrals gave the characteristic shapes of curves already described and also agreed closely in absolute value with the experimental results. This additional information appears to us to corroborate the hypothesis that for such links the presence of low ducts does not affect the propagation conditions, except that a pronounced change in refractive index near the sea surface might alter the mean gradient of refractive index in the atmosphere.

We should like to emphasize that statistical analysis of the type outlined briefly above is a powerful method for resolving the complicated phenomena of meteorological and topographical nature which control the propagation of short radio waves. Mr. J. A. Ratcliffe[2] has already pointed out how statistical analysis may be used to study the propagation of longer radio waves through the ionosphere.

We are indebted to the Chief Scientist, Ministry of Supply, for granting permission to publish this note.

J. S. McPetrie
B. J. Starkey

Signals Research and
Development Establishment,
Christchurch, Hants.
Jan. 7.

[1] *Nature*, **162**, 818 (1948).
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Energy Spectrum of Secondary Electrons from μ-Meson Decay

Recent experimental results are strongly in favour of the decay of the μ-meson into more than two particles[1]. The energy spectrum of the electron emitted in the disintegration of the μ-meson at rest has been calculated on the following assumptions. The μ-meson has spin ½; it decays spontaneously according to the scheme $\mu^{\pm} \rightarrow e^{\pm} + \lambda + \nu$, where e indicates an electron, ν a neutrino and λ a neutral particle of spin ½ with a mass either non-vanishing (μ^0 of Powell[2]) or more probably zero, in which case it can be identified with a neutrino.

The process is treated by ordinary perturbation theory; each charged particle is described by a plane wave according to Dirac's hole theory. For the neutral particles two theories are used: (*a*) Dirac's hole theory, assuming that it is possible to distinguish the particles from the anti-particles; (*b*) Majorana's abbreviated theory[3], which identifies the particles and the anti-particles.

The Hamiltonian of interaction is the same as for the β-decay theory, with five interaction constants.

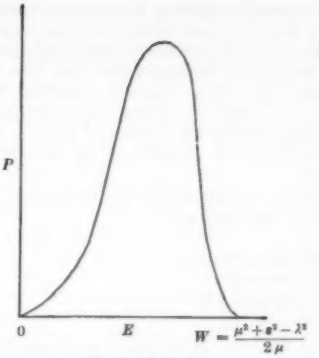

Fig. 1. $\lambda \neq 0$

All possible ways of constructing a Hamiltonian of this kind are shown, by suitable linear transformations of the five interaction constants, to be equivalent.

For the probability $P(E)dE$ of observing the electron with a total energy between E and $E + dE$ ($\varepsilon \leqslant E \leqslant W = \frac{\mu^2+\varepsilon^2-\lambda^2}{2\mu}$, where μ, ε, λ are the rest masses in energy units) we find the following results. If $\lambda \neq 0$, both descriptions *a* and *b*, chosen independently for the two neutral particles, lead to the same result; $P(E)$ has the shape of Fig. 1, characterized by the factor $\sqrt{E^2-\varepsilon^2}\,(W-E)^2/[2\mu(W-E)+\lambda^2]^2$.

If $\lambda = 0$ (the neutral particles are two neutrinos of zero rest mass) we have two different cases:

(1) All neutrinos are described by Dirac's hole theory, and the two emitted neutrinos are one particle and one anti-particle experimentally distinguishable (for example, by the sign of their magnetic moment); then

$$P(E) = \frac{\sqrt{E^2-\varepsilon^2}}{3\hbar(2\pi\hbar^2c^2)^3}\,[3\mu E\,(W-E)\,K_1 + 2\mu\,(E^2-\varepsilon^2)K_2 + 3\mu\varepsilon\,(W-E)\,K_3 + \varepsilon\,(E^2-\varepsilon^2)\,K_4],$$

with

$$K_1 = g_1^2 + 2(g_2^2+g_3^2+g_4^2) + g_5^2,\quad K_2 = g_3^2 + 2g_2^2 + g_4^2,$$
$$K_3 = g_1^2 - \tfrac{3}{2}(g_2^2-g_4^2) + g_5^2,\qquad K_4 = -\tfrac{1}{2}\,(g_2^2-g_4^2).$$

The terms proportional to K_3 and K_4 are very small (Fig. 2).

(2) Either all neutrinos are described by Majorana's abbreviated theory, or all neutrinos are described by Dirac's hole theory, and the two emitted neutrinos

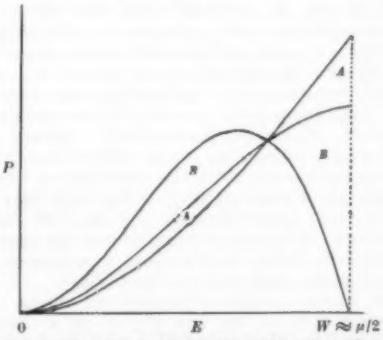

Fig. 2. $\lambda = 0$. When the g's take on all possible values, the curve sweeps the whole areas A and B in case 1, and only the area B in case 2

are either two particles or two anti-particles; in each case the two emitted neutrinos are identical. The application of the exclusion principle gives the same results as above, except that now the interactions in g_1 and g_3 do not appear. (In the formula put $g_2 = g_3 = 0$) (Fig. 2). From the experimental value of the mean life of the μ-meson, the g's are found of the same order of magnitude as that of the Fermi constant[4].

I wish to thank Prof. L. Rosenfeld for helpful advice and discussions.

LOUIS MICHEL

Physics Department,
University,
Manchester 13.
March 23.

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The Resolution/Brightness/Contrast Sensitivity of the Eye in Certain Forms of Picture Reproduction

AT a meeting of the Physiological Section of the British Association last September, Prof. H. H. Hartridge described some experiments on visual acuity, using a Landolt ring as test object, which showed little difference in resolving power when the ring was illuminated with red, green or blue light. The results of some experiments which we have been carrying out appear to be relevant to this point.

We are investigating the resolution/brightness/contrast sensitivity of the eye in certain forms of picture reproduction. In these tests, the observer's eye is adapted by a 'picture' and surrounding field of constant specified brightness in any given experiment. A small part (approximately one-seventh of the picture height) in the centre of the field is then varied in any desired manner. In one set of experiments, this test patch was masked with tricolour red, green and blue filters in turn, the remainder of the picture being in black and white. The brightness of the coloured test patches were adjusted to correspond to those of the reproducing primaries in a three-colour additive system. At brightnesses appropriate to the white high-lights of the picture, we have found that visual acuity is slightly less for blue light than for red, which in turn is practically the same as for green. These conditions are analogous to Hartridge's experiment, in which a black test object is printed on a white card illuminated by red, green or blue light, and the results confirm his findings. At successively lower brightnesses of the test field, however, visual acuity is, of course, progressively reduced for all three colours. The drop in the case of blue, however, is much more rapid, with the result that a blue/green acuity ratio of approximately 0·8 at high-light brightness falls to approximately 0·3 at 1/32 high-light brightness. Adaptation conditions must apparently, therefore, be taken into account in assessing experimental results such as Prof. Hartridge's.

The above results represent the mean of five observers viewing the test picture at a distance of four times its height and at a high-light brightness of 10 ft. lamberts.

Our results confirm that similar discrepancies occur when comparing the performance of various monochrome imaging systems. A comparison made under high-light conditions (maximum brightness and contrast of the test pattern) can be quite misleading. It is necessary to give a range of resolution/brightness/contrast data if a fair comparison is to be made. This fact is, fortunately, now becoming more widely understood.

L. C. JESTY

Cinema-Television, Ltd.,
Worsley Bridge Road,
Lower Sydenham,
London, S.E.26.
Dec. 14.

Behaviour of Crystal Boundaries in Aluminium and its Alloys during Melting

WE have recently been engaged on an examination of the mechanical properties at temperatures in the region of the solidus of super-purity aluminium and high-purity alloys in the three binary alloy systems, aluminium–copper, aluminium–iron and aluminium–manganese. In this work, tensile tests have been carried out at elevated temperatures in a modified Hounsfield tensometer on small cylindrical test pieces of the alloys, the technique and apparatus being similar to those used by Singer and Cottrell[1].

The analysis of the super-purity aluminium (99·988 per cent aluminium) and that of a representative alloy based on this material was as follows:

Material	Si%	Fe%	Cu%	Mg%	Al%
Super-purity aluminium	0·005	0·0015	0·0005	0·005	remainder
Aluminium–3% copper alloy	0·006	0·005	3·07	0·006	remainder

Each cast bar from which test pieces were machined was annealed for approximately six weeks at a temperature some 25° C. below the equilibrium solidus temperature of the particular alloy considered. The annealed test-pieces were tested at temperatures between 400° C. and the temperature at which each contained so much liquid that all coherence was lost. The tensile strength of the alloys gradually decreased, and the ductility (measured as percentage elongation and reduction of area) increased correspondingly with increase in the temperature of testing up to a temperature some few degrees below the solidus temperature determined from the equilibrium constitutional diagram. At this temperature the elongation and reduction of area decreased suddenly to zero, and the strength dropped rapidly to a small finite value which was retained up to a temperature between that of the solidus and liquidus. The surfaces of fracture in the specimens having no ductility were intercrystalline and had a glazed and rounded appearance.

It seems unlikely that the glazed type of fracture can be accounted for on the basis of viscous flow of the crystal boundary material at the high temperature of testing, since the occurrence of fracture due to viscous flow would not be expected to be accompanied by a sudden fall in strength. It has therefore been assumed that the sudden loss of ductility with increase in the temperature of testing, and the appearance of the glazed type of fracture, are associated with incipient melting at the crystal boundaries.

With super-purity aluminium and with all the alloys examined in the three binary alloy systems,

it was observed that the temperature of incipient melting of the crystal boundaries was some 4° C. below that of the metal in the mass as determined from the equilibrium constitutional diagram. The following figures, which show the depression of the temperature of incipient melting in super-purity aluminium with the addition of 0·01 per cent of an element, were obtained from the relevant binary constitutional diagrams and indicate that the observed depression of 4° C. cannot be ascribed to the presence of small traces of impurity in the metal.

	Element			
	Si	Fe	Cu	Mg
Depression of temperature of incipient melting, °C.	0·6	1·7	0·23	0·16

Iron has the greatest effect in depressing the temperature at which incipient melting occurs in super-purity aluminium; the presence of iron in super-purity aluminium and in the aluminium–copper and aluminium–manganese alloys is unlikely to account for the observed pre-melting phenomenon, however, since even in an aluminium–iron alloy of eutectic composition, melting began in the crystal boundaries at a temperature some 4° C. below the equilibrium eutectic temperature.

An examination of the appropriate ternary equilibrium diagrams also failed to provide an explanation of the pre-melting phenomenon on the basis of the amount of impurity in the binary alloys.

The present results are of interest in relation to the work of Chaudron, Lacombe and Yannaquis[2]. If it is assumed that a reasonably uniform temperature gradient of 10° C. existed between the ends of the flat test strip used by these workers, it can be deduced that in their experiments melting of the crystal boundaries in super-purity aluminium occurred at a temperature some 3–4° C. lower than melting in the body of the crystals. This is in agreement with our results.

There would seem to be two possible explanations for the fact that the temperature at which melting begins in the crystal boundaries is lower than that normally accepted for the same metal in the mass. The first is that advanced by Chalmers[3], who postulated that the crystal boundaries have a lower melting point than the crystals themselves because of the interatomic tensional stresses due to the atomic disorder in such regions. The second possible explanation is that the presence of foreign atoms of greater or smaller atomic diameter than the aluminium atom may lower the interatomic stresses in the disordered boundary region, and this may increase the tendency for impurities to concentrate at the crystal boundaries and to remain there even after the mass has been annealed for some considerable time. The impurities concentrated at the boundaries in this way may reduce the temperature of incipient melting of the boundary material in the normal manner.

Further investigation of this phenomenon is now in progress, and it is hoped to publish the results obtained in this work in the near future.

W. I. Pumphrey
J. V. Lyons

School of Industrial Metallurgy,
University of Birmingham.
Jan. 27.

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Fine Structure of Slip-Zones

Recently, Heidenreich and Shockley[1] have reported that, in single crystals of aluminium, deformed at room temperature, the slip-lines can be resolved by the electron microscope into a cluster of finer lines. These lines are found to be the boundaries of elementary glide lamellæ some 200 A. thick, which have slipped over each other a distance of about 2000 A. It is convenient to distinguish between the clusters usually called 'slip-lines' and the finer lines composing them by calling the former 'slip-zones' and the latter 'elementary slip-lines'. It would be interesting to find if this fine structure is fundamental to the slip process in metals beyond the individual case investigated by Heidenreich and Shockley; in particular, one would like to see if slip-zones in the hexagonal metals can be resolved.

Unfortunately, it is not possible as yet to repeat the experiment with other metals, since only from aluminium can replicas be obtained with sufficient resolution. One can, however, while still using aluminium, vary the temperature at which slip occurs as well as the type and rate of deformation. In the experiments described here, slip-zones formed at various temperatures from that of liquid air to 500° C. have been studied.

Spectroscopically pure aluminium (99·995 per cent aluminium) was recrystallized to give a variety of grain sizes and then polished entirely electrolytically. The orientation of the grains was found approximately by etching in Lacombe's[2] etchant (aqua regia + 5 per cent hydrofluoric acid), which gives small, neat pits, the sides of which appear to be accurately {100} planes. Intersections of the slip-zones with these pits give a three-dimensional picture of the slip process and enable calculations of slip angles and distances to be made with sufficient accuracy without the use of stereos.

The polished aluminium specimens are then deformed by compression or bending at various temperatures. As is well known, for a given amount of deformation, the spacing and apparent breadth of the slip-zones, as seen in the light microscope, increase with temperature because, as it is usually expressed, there is less work-hardening at the higher temperatures and so slip can proceed farther on the same glide plane, or group of planes. The light microscope also shows that at liquid–air temperature the slip-zones are very straight and become more curved as the temperature of deformation is increased.

Electron-micrographs, made by means of oxide replicas, show that at all temperatures the slip-zones seen optically can be resolved into elementary slip-lines as found by Heidenreich and Shockley, and the surprising result is that, at all temperatures, the slip unit is approximately the same; the only difference lies in the grouping of these elementary lines into clusters (zones). Thus, at all temperatures between − 180° C. and 500° C., the slip-zones are found to be composed of lamellæ some 200–800 A. thick and bounded by {111} planes. Each lamella has slipped about 2000 A. relative to its neighbours, this distance being measured in the slip-plane and slip direction.

For example, typical values at about 15 per cent deformation (measured in the conventional way) are:

At	−180° C.	the zones are spaced	¼–1 μ	apart and contain	1–2 lines
,,	20° C.	,,	,,	,,	about 2 μ	,,	,,	,,	3–4	,,
,,	250° C.	,,	,,	,,	,,	4 μ	,,	,,	,,	5–6	,,
,,	500° C.	,,	,,	,,	,,	10 μ	,,	,,	,,	~12	,,

At higher deformations more sub-lines are found in each cluster, while at very low deformations the zones are widely spaced at all temperatures and contain only one of the elementary slip-lines.

Thus the progress of increasing deformation is different at different temperatures: first, at all temperatures, one-line zones are formed at small deformations, and increasing deformation creates new zones until a minimum spacing depending on temperature in the manner given above is reached. Thereafter increasing deformation results in increasing multiplicity in the zones.

The curved form usually found in slip-zones formed at high temperatures means that slip can be transferred from one of the elementary slip-planes to another parallel one by means of a third, intersecting, slip-plane; the direction of slip being the same in both systems. Thus, elementary slip-lines may appear to be stepped, and this means that the elementary slip-planes are enabled to interweave at high temperatures and even at room temperature, although the phenomenon has not been observed at liquid-air temperature. A similar effect has been found in brass by Maddin, Mathewson and Hibbard[3], but here the scale was large enough to be visible in the light microscope.

The thickness of the elementary glide lamellæ is more variable than their relative displacement; neither does it appear to be directly related to temperature. The most common thickness is about 200 A., but values up to 800 A. are found, particularly at the higher temperatures. The origin of the lamellæ is uncertain; but it is interesting to note that studies of shadowed replicas of unetched electro-polished aluminium surfaces reveal a furrow-like structure—rather like the thermal etch structure studied on the surface of silver by Chalmers, King and Shuttleworth[4], but sometimes more irregular, depending on the crystallographic nature of the surface and the direction of shadowing. The observed size of this furrow structure is of the same order as the observed thickness of the elementary slip lamellæ and, in some cases, has the same direction.

ARTHUR F. BROWN

Cavendish Laboratory,
Cambridge.
May 2.

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Occurrence of Sepiolite in the Lizard Serpentines

DURING an excursion to Lizard Head (Cornwall), we examined in some detail the exposures of serpentine at the water-edge. At a point situated about 500 metres south-east of Kynance Cove Hotel, our attention was attracted by the presence of numerous white veinlets, standing out against the dark colour of the rock. One of them, almost vertical, about 5 cm. wide, shows on its upper face a thin layer of a fibrous mineral. The chemical analysis, differential thermal curve, and graph of weight-loss as a function of temperature for this mineral are similar to those obtained with Ampandrandava sepiolite. The few differences observed can be attributed to the presence of calcite associated with the Kynance Cove mineral.

X-ray diagrams show the complete identity of the two silicates. We may emphasize that in both cases a fibre diagram can be obtained by using suitably chosen fragments. The Kynance Cove mineral is thus a sepiolite; an identification which is confirmed by microscopic examination, which shows the following optical properties: the fibres have straight extinction with positive elongation; $\gamma = 1{\cdot}52$; biaxial negative with small axial angle. This method also establishes the fact that the sepiolite has been formed after the calcite, which fills the larger part of the vein. The silicate fibres surround the grains of calcite, which are rounded off at the contact as if they had been corroded during the formation of the sepiolite.

This allows the conclusion to be drawn that the sepiolite, the most recent mineral of the series, is a secondary product resulting from the alteration of the encasing serpentine.

Our thanks are due to Dr. Clark and Dr. Parker, of English Clays Lovering Pochin and Co., Ltd., St. Austell, who helped in arranging the excursion, and to Dr. MacEwan (Rothamsted Experimental Station), who translated this note into English.

S. CAILLÈRE

Muséum National d'Histoire Naturelle,
Paris.

S. HÉNIN

Centre National de Recherches Agronomiques,
Versailles.

A Saline Agglutinating Kell Antibody

THREE examples of anti-Kell are known[1]; but all are of the incomplete variety necessitating the use of Coombs's reagent to develop their action. The serum here reported is the first saline agglutinating anti-Kell discovered.

It was found in a multiparous woman who had been delivered of a macerated, stillborn fœtus. She was Group *O*, *Rh* positive, genotype *CDe/cDE*, *M*, *P*, Lewis negative, whereas her husband was Group *B*, *Rh* positive, genotype *CDe/CDe*, *M*, *N*, *p*, Lewis positive. Her serum contained an antibody, active in saline at 37° C. with a titre of 4, not related to the *ABO*, Rhesus, *MNS*, *P*, Lewis and Lutheran systems, and agglutinating Kell positive cells. Unfortunately, no blood was sent from the macerated fœtus.

Race[2] has described the Kell antigen, and it would appear to be related to hæmolytic disease of the newborn, as in the first case described[3] the child showed clinical signs and symptoms of the disease, with a positive Coombs's test, and responded to transfusion therapy, whereas the present case terminated in a macerated fœtus.

The serum is active in albumen at 37° C., is destroyed by heating at 70° C. for 10 min. and does not contain an incomplete antibody demonstrable by Coombs's reagent after heat treatment.

Tests with this serum against 566 unselected *O* bloods shows the Kell antigen to occur in 7·2 per cent of English people.

I. DUNSFORD

National Blood Transfusion Service,
Sheffield.

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EXCHANGE OF MATERIALS IN A LAKE AS STUDIED BY RADIOACTIVE PHOSPHORUS

By Prof. C. C. GOFFIN, Dr. F. R. HAYES, L. H. JODREY and S. G. WHITEWAY
Departments of Chemistry and Zoology, Dalhousie University, and
Fisheries Division, Nova Scotia Department of Trade and Industry

SHORTLY after dawn, on July 1, 1948, the weather being fine and calm, 100 millicuries of radioactive phosphorus (P^{32}) was added to the surface of an acid bog lake of area 0·3 hectare (0·8 acre) and depth 6 metres, situated near Halifax, Nova Scotia. The run-off was small and the margin almost wholly surrounded by sphagnum. The lake had a well-developed zone of cold, stagnant water at the bottom with a thermocline above. Oxygen reached two-thirds saturation at the surface, was scarce at three metres depth, and absent at five metres. The only native fish observed were *Fundulus diaphanus* and *Notemigonus crysoleucas*. Before the experiment, some four thousand *Fundulus* were added to the lake for later sampling.

The added phosphorus-32, being mixed by means of an outboard motor, was initially distributed evenly to a depth of some 1·5 metres. Within two hours there was a specific removal of phosphorus-32 from the surface water, so that the maximum concentration was one metre down. This is believed to be related to the overhanging margins of floating sphagnum. The peak had disappeared 13 hours after zero time. Penetration of added phosphorus into the lake depths was very slow, and even at the end of eight weeks the material was only doubtfully detected at the bottom. There was no evidence of active uptake by bottom mud.

The total phosphorus-32 in the lake declined for 8 hours, rose again at 13 hours, and afterwards declined steadily. The uptake by sphagnum showed a reciprocal relation, with a peak at 8 hours and a trough at 13. Assuming that the increase in lake phosphorus is due to liberation by sphagnum, it is possible to calculate the quantity of actively growing, or at least participating, sphagnum, which works out at 1·5 metric tons.

Although no great confidence is to be placed in this value until checked, the method opens interesting possibilities in aquatic studies. The uptake by sphagnum exhibited a second peak at about three weeks. It appears reasonable to think that sphagnum possesses two mechanisms of phosphorus uptake, the first of which might be due to physical processes unconnected with active cell metabolism. The second peak might be due to the incorporation of phosphorus into the protoplasm in some permanent way.

Samples of plankton, collected from time to time, turned out to be practically pure cultures of *Diaptomus*. Uptake of phosphorus-32 was apparent within five hours and reached a peak in eight days.

The situation as regards plankton is quite different from that of sphagnum, the former being composed of animals whose lives are of short duration, so that the counts of phosphorus-32 do not represent the conditions in a single generation which lives through the summer, but in several generations.

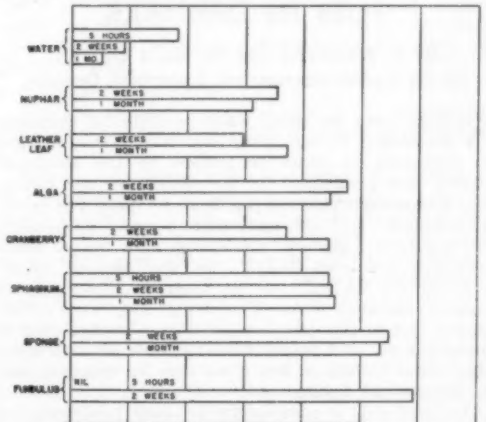
No phosphorus-32 could be found in fish until some fifty hours after the beginning of the experiment. Such a late beginning of deposition contrasts with the early uptake by plankton, and suggests that different mechanisms may be operating. In the case of the plankton, it is easy to imagine that inorganic phosphate is taken up directly, while in the fish the time-lag suggests that the phosphorus-32 must first be taken up by the plankton organisms and incorporated into their tissues. The fish would then receive phosphorus by eating the plankton.

No phosphorus-32 was detected in the leaves of marginal rooted organisms until a fortnight after the experiment began. The delayed appearance suggests that phosphorus reached the leaves via the roots.

Four counts on the alga *Batrachospermum* were also made between nine days and eight weeks from zero time. No trend was discernible, the maximum value being already established by the time the first count was made. It is reasonable to think that such a form would take up phosphorus quickly; but proof is not at hand.

The intensity of phosphorus assimilation can be calculated in two ways. First, we may give the absolute uptake, which is the ratio of the maximum observed value for an organism compared to the maximum value for the same weight of water. The relationship can be derived from Fig. 1, in which it is seen that animals take up more than plants; higher animals more than lower animals; and lower plants more than higher plants. As regards the last statement, however, it must be remembered that the measurements on whole algæ and on all exposed parts of sphagnum are not strictly comparable to the leaf counts made on higher plants. It was observed that the growing tips of a higher plant gave greater counts than leaves.

A second calculation gives the relative uptake by answering the question: How much phosphorus-32 can be taken up in proportion to the total phosphorus already in the organism? This is a measure of the rate of turnover of phosphorus.



Counts per min. per 100 gm.

Fig. 1

	Ratio of phosphorus-32 to total phosphorus in organism × 10^{-7}
Alga	660
Sphagnum	10
Cranberry	3
Leather leaf	1
Yellow water lily	2
Sponge	500
Zooplankton	13
Fish	2

As the table shows, maximal values are observed for alga and sponge, the simplest forms dealt with. Next, but much lower, come sphagnum and *Diaptomus*. Fish and higher plants make up a still more sluggish group. The high value for *Diaptomus* as compared with fish is, perhaps, related to (*a*) the short life of a generation of plankton organisms, making a rapid turnover of material necessary, and (*b*) the mass of inorganic phosphate in a fish skeleton, which is renewed fairly slowly. This slow exchange is demonstrable by radioautographs. No specific deposition was observed at nine days, but by fifteen days *Notemigonus*, as shown in Fig. 2, had laid down the phosphorus-32 in the axial skeleton and at the bases of the fins.

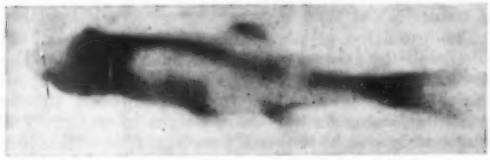


Fig. 2

In both systems of measuring intensity of uptake, the sphagnum ratios are below some of the others. Nevertheless, sphagnum is believed to be the main agent for the removal of phosphorus-32 in this lake, because of its great abundance.

A full account of this work will be published elsewhere.

D.D.T.-RESISTANCE IN HOUSE-FLIES IN DENMARK

By J. KEIDING and H. VAN DEURS
Statens Skadedyrlaboratorium, Springforbi, Denmark

SINCE early in 1944, when insecticidal products containing D.D.T. were first put on the market in Denmark, a great proportion of the available D.D.T. has been used for fly control on our farms. The commonest practice has been to spray the inside of cowsheds, pigsties, etc., with a water suspension or emulsion, usually containing not more than 0·05–0·1 per cent D.D.T., at the rate of about 0·1–0·2 gm. D.D.T. per sq. metre. The spraying must be carefully done. This dosage may seem rather small; but in the first year or two it worked well in most places, and many farmers were able to keep their farm buildings free from flies by spraying two or three times a year.

In 1944 only a comparatively small percentage of the farmers in Denmark used D.D.T. against flies. In 1945 many dairies started control of flies by regular spraying of the cowsheds, etc., of their suppliers, and in 1946 this organised control was in action all over the country.

Already in 1946 there were some isolated complaints of failing effect, and in the summer of 1947 the fly control failed in a great many places. It was attributed to careless spraying, as well as to the unusually hot summer, the effect of the high temperature being an increased production of flies in the dung-heaps, etc., a decrease in the toxicity of D.D.T. to insects[1], and possibly a more rapid deterioration of the deposits on limewashed surfaces[2].

Early in 1948 we received accounts from various places of flies which "could not be killed with D.D.T.", even with much higher dosages than usually recommended. Houseflies (adults, pupæ and larvæ) were collected from six of these places and the strains so obtained (r_1, r_2, r_3, etc.) were bred by a modified N.A.I.D.M. procedure[3] in parallel with the non-resistant laboratory strain (lab) which has been bred from flies taken on a farm near Copenhagen in October 1945.

The results of the first laboratory tests showed unmistakably that all six *r*-strains were several times more resistant to D.D.T. than the laboratory strain.

In order to simulate our common type of stable wall, lime-washed flowerpots (6 in. in diameter) were used as test containers and sprayed (or dusted) evenly on the inside with measured dosages of insecticide. Most of the tests were made by exposing the flies continuously to the treated surface, the opening of a container being covered with a loose glass plate during tests.

The first results were confirmed in a great number of laboratory tests with several D.D.T. residual-spray formulations, dusts, etc., using later generations of the *r*- and lab-strains. Whereas the lab-flies might be 'knocked down' and moribund within an hour or even half an hour with a dose of 2 gm. D.D.T. per square metre, some of the *r*-flies could live for days exposed to the same surface; we have even had *r*-flies, white all over with 33 per cent D.D.T.-dust, which lived for several days. In the accompanying table, examples are given of some of the initial tests with three of the *r*-strains and the lab-flies.

Reaction of lab-flies and *r*-flies to continuous exposure to residual-spray deposits of D.D.T. on limewash

Insecticide dose	Flies Strain	Number	Av. percent. of flies knocked down after ½	1	3	6	24	48 hr.
D.D.T. 5% oil-solution 2 gm. D.D.T./m.²	lab	29+26+31	94	100				
	r_1	19	0	0	0	0	11	11
	r_2	25+18	0	2	37	40	58	72
	r_3	22+11	0	0	3	6	18	36
D.D.T. 25% wettable powder 2 gm. D.D.T./m.²	lab	22+29+34	87	100				
	r_1	22	0	0	0	0	68	68
	r_2	40+20	2	17	38	47	98	98
	r_3	22+7	0	7	21	24	66	76
D.D.T. 25% wettable powder 1 gm. D.D.T./m.²	lab	25+31+32	90	100				
	r_1	21	0	0	0	10	43	52
	r_2	42+30	0	6	28	40	100	
	r_3	20+7	0	0	7	7	52	70

The resistance was not quite the same in all the *r*-strains, as one might expect; in later generations, however, the differences have become slight, as the resistance of the most resistant strains has decreased, although it is still remarkably high after seven to nine generations. In order to secure a highly resistant strain, some flies were bred in a cage treated with a whitewash containing about 0·3 per cent D.D.T.

Since the demonstration of D.D.T.-resistance in the laboratory, we have had numerous reports of

D.D.T. failing to control flies even with high dosages. Although no thorough survey has been made, we have enough information to show that populations of flies resistant to D.D.T. in greater or less degree occur commonly in most parts of Denmark, and dominate in some districts.

In almost all cases of which we have information, the flies did not show high resistance during the first D.D.T.-treatments. In some cases resistance was noticed the year after the first D.D.T.-treatment; but, in general, resistant flies were not observed until after two to three years of successful control with D.D.T.

Obviously the resistance has developed simultaneously in a great number of populations (the flies of a single farm probably often acting as an isolated population), and to us the most reasonable explanation is that these populations already contained a small proportion of flies with genes determining D.D.T.-resistance; through a number of generations this proportion has increased rapidly as a result of a continuous selective extermination of non-resistant flies, until finally the entire population consists of the resistant genotype.

In our attempts to find ways of controlling the D.D.T.-resistant flies, tests have been made with six contact insecticides, which may be grouped as follows:

(1) Compounds with a D.D.T.-like structure:
'Methoxychlor': 2,2-di-*p*-anisyl-1,1,1-trichloroethane;
'Gix': 2,2-*bis*(*p*-fluorophenyl)-1,1,1-trichloroethane;
D.D.D. or T.D.E.: 1,1-*bis*(*p*-chlorophenyl)-2,2-dichloroethane.
(2) Other chlorinated cyclic hydrocarbons:
Benzene hexachloride (B.H.C.): 1,2,3,4,5,6-hexachloro-*cyclo*hexane;
'Chlordane': 1,2,4,5,6,7,8,8-octachloro-4,7-methano-3*a*,4,7,7*a*-tetrahydroindane;
'Toxaphene': a chlorinated camphene.

Using the same technique as mentioned above, but supplementing it with tests with restricted time of exposure, we found that the *r*-flies showed a similar resistance towards the insecticides of group 1 as towards D.D.T., the difference in 'knock-down' and kill between the *r*- and lab-flies being very considerable. Towards group 2, on the other hand, the *r*-flies and the lab-flies reacted uniformly (unlike the D.D.T.-resistant flies developed in the laboratory in Orlando, U.S.A.[4]).

Practical trials have already shown that benzene hexachloride can be used to control D.D.T.-resistant flies, although the residual effect in many cases has been rather short-lived. In fact, this substance has already been used on many Danish farms.

In addition to the publications on D.D.T.-resistant flies developed in laboratories in the United States[5,6,7], we have been able to find published reports on only two naturally occurring cases of D.D.T.-resistance in house-flies, these reports coming from Sweden[9] and Italy[9,10]. From verbal information and from correspondence we know, however, that similar phenomena have been noted in other places, where D.D.T. has been used against flies for some time, not only in Europe but also in the United States.

[1] Lindquist, A. W., Wilson, H. G., Schroeder, H. O., and Madden, A. H., *J. Econ. Ent.*, **38**, 261 (1945).
[2] Van Deurs, H., *Ugeskrift for Landmænd*, **93**, 71 (1948).
[3] Anon., *Soap Blue Book* (1945).
[4] Wilson, H. G., and Gahan, J. B., *Science*, **107**, 276 (1948).
[5] Lindquist, A. W., and Wilson, H. G., *Science*, **107**, 276 (1948).
[6] Blickle, R. L., Capelle, A., and Morse, W. J., *Soap Sanit. Chem.*, **24** (8), 139 (1948).
[7] Barber, G. W., Starnes, O., and Starnes, E. B., *Soap Sanit. Chem.*, **24** (11), 120 (1948).
[8] Wiesmann, R., *Mitt. Schweiz. Ent. Ges.*, **20**, 484 (1947).
[9] Saccà, G., *Riv. di Parassit.*, **8**, 127 (1947).
[10] La Face, L., *Riv. di Parassit.*, **9**, 199 (1948) (added in proof).

AWARDS OF STALIN PRIZES IN THE U.S.S.R.

A LIST of awards of Stalin Prizes for 1948, for distinguished achievement in the U.S.S.R., has been published in *Izvestia* of April 9; the awards are of 200,000 roubles (first class) or 100,000 roubles (second class). A translation of the names of recipients and grounds of awards is given below:

A. Physico-Mathematical Sciences

First-class prizes: S. N. Vernov, professor in the University of Moscow, for the study of cosmic rays; Academician M. A. Lavrentiev, for theoretical studies on hydrodynamics; G. D. Latyshev, corresponding member of the Academy of Sciences, for the study of the atomic nucleus.

Second-class prizes: G. A. Grinberg, corresponding member of the Academy of Sciences, for theoretical studies in mathematical physics relating to electricity and magnetism; L. V. Kantorovich, professor in the University of Leningrad, for work on functional analysis; I. A. Khvostikov, professor in the military aero-engineering Academy, for studies on atmospheric optics.

B. Technical Sciences

First-class prizes: G. V. Kourdumov, corresponding member of the Academy of Sciences, for work on metallography; G. I. Petrov, member of the Central Institute of Aviation Motors, for work on gas dynamics.

Second-class prizes: I. I. Kirillov and S. A. Kantor, professors in the Leningrad Polytechnic Institute, for joint work on the theory and construction of steam turbines; A. P. Krylov, M. M. Glogovsky, M. F. Mirchink, N. M. Nikolaevsky and I. A. Charnom, members of the Moscow Petroleum Institute, for a joint work on the scientific exploitation of petroleum deposits; Y. M. Parkhomovsky, N. V. Alkhimovich and L. S. Popov, members of the Central Aero-Hydro-dynamic Institute, for theoretical and experimental studies in mechanics; E. P. Popov, professor in the Military Aero-Engineering Academy, for studies in the theory of elasticity; K. A. Rakhmatulin, Academician, Usbek Academy of Sciences, and professor in the University of Moscow, for work on the theory of waves; M. I. Yanovsky, corresponding member of the Academy of Sciences, for work on steam turbines.

C. Chemical Sciences

First-class prize: Academician B. A. Kazansky, for work on the catalytic transformations of hydrocarbons.

Second-class prizes: B. A. Dolgoplosk, professor in the Synthetic Rubber Institute, for work on polymerization; V. V. Korshak, professor of the Moscow Chemical-Technological Institute, for work on high-molecular compounds; S. S. Perov, member of the Agricultural Academy, for work on the biochemistry of proteins.

D. Biological Sciences

First-class prize: Academician T. D. Lysenko, president of the Agricultural Academy, for work in biology as presented in a book entitled "Agrobiology" published in 1948.

Second-class prizes: L. S. Davitashvili, member of the Georgian Academy of Sciences, for a book entitled "History of Evolutionary Palæontology from Darwin to the Present Time", published in 1948; V. A. Movchan, professor in the University of Kiev,

for work on the biology of the carp; M. V. Fedorov, professor in the Moscow Agricultural Academy, for work on the fixation of atmospheric nitrogen by soil bacteria.

E. Agricultural Sciences

First-class prizes: K. E. Bakhtadze, doctor of agricultural sciences, for work on the biology of the tea plant; S. M. Boukasov and A. Y. Kameraz, members of the Plant-growing Institute, for joint work on selection of potatoes.

Second-class prizes: A. M. Dmitriev, professor in the Moscow Agricultural Institute, for work on meadows; V. P. Timofeev, professor of forestry, for work on forestry.

F. Medical Sciences

First-class prize: T. P. Krasnobaev, member of the Medical Academy, for work on bone and joint tuberculosis in children.

Second-class prize: P. F. Zdrodovsky, member of the Medical Academy, for work on infectious diseases.

G. Economic Sciences

First-class prize: P. I. Lyashchenko, corresponding member of the Academy of Sciences, for studies in the economics of the U.S.S.R.

H. Legal Sciences

Second-class prize: A. V. Venediktov, professor in the University of Leningrad, for work on the State socialistic property.

I. History-philological Sciences

First-class prizes: B. A. Rybakov, member of the Institute for the Study of Material Culture, for work in the history of crafts in Old Russia; S. P. Tolstov, professor in the University of Moscow, for historical-archæological work.

GROWTH OF SMALL BUSINESSES

THE paper, "Problems of Growth in Industrial Undertakings", presented before the British Institute of Management by Colonel L. Urwick on January 12, has now been issued as No. 2 of the Winter Proceedings, 1948–49, together with a report of the discussion, including written contributions. Colonel Urwick emphasizes that re-organisation is a continuous process requiring specific provision, and that one of the main tasks of re-organisation is to keep the different parts of a business in balance. Growth is essential if individuals employed by an undertaking are to grow as individuals, and balance is essential in terms of time as well as in terms of the relative strength of parts. Col. Urwick considers that factual research is needed to determine whether growth should be continuous or interspersed by periods of consolidation.

In the development of a unit business there are three critical points. First, that at which the owner-manager, or chief executive, who has, up to that point, dealt single-handed with all the major functions of the business, has to start delegating full responsibility for the initiative, as opposed to routine supervision, in one of the major functions. This involves both a personal and a personnel problem, and these problems recur with the delegation of full initiative in each of the major functions. Second is the point at which the chief executive has to start delegating full responsibility for initiative in the ancillary functions. Again, the problem recurs as each of the ancillary functions is specialized. Since such functional or specialized control cuts across ordinary line authority, its development greatly increases the volume of co-ordination required; and the third critical point is accordingly that at which this additional co-ordination compels the chief executive to take special measures to prevent himself being overwhelmed.

One measure Col. Urwick suggests here is the use of officials in a true 'staff' capacity; but he points out that, at each stage, organisation tends to lag behind requirements and the chief executive to exceed his span of control, either because he is temperamentally unable to delegate or because he is unaware of the importance of leaving himself sufficient time to lead his team. Col. Urwick's paper is illustrated with a series of organisation charts setting out the development of a one-, two-, three- or four-man business into complete functional control of a unit business.

EFFECTS OF LIGHT INTENSITY AND DAY-LENGTH ON REPRODUCTION IN THE ENGLISH SPARROW

THE effects of light intensity and day-length on reproduction in the English sparrow have been studied quantitatively in the laboratory by George A. Bartholomew, jun. (*Bull. Mus. Comp. Zool., Harvard*, 101, No. 3; Feb. 1949). He has also considered the ecological significance of these factors in controlling the breeding season of this species in Nature.

A quantitative evaluation of the effects of five different intensities of light was obtained by exposing sparrows to these light intensities during uniform day-lengths of sixteen hours. In the male, 10 foot-candles was more effective than lower intensities and was as effective as either 52 or 244 foot-candles in causing gonadal development in the winter. During the autumn, however, 10 foot-candles was much less effective than 244 foot-candles, indicating a seasonal variation in response to light intensity. The minimum light intensity which caused full spermatogenic activity was 0·7 foot-candles; but slight testicular activity was caused by 0·04 foot-candles.

Experiments testing the effect of differing light intensities during sixteen-hour days showed that in the female, as in the male, gonadal growth increased with increasing intensity. These experiments, limited to the winter, showed that after forty-six days the highest intensity used (244 foot candles) caused more ovarian development than any of the lower intensities. By eighty-six days, however, gonadal regression had set in and ovarian size was no longer correlated with light intensity. This confirms the observations of previous investigators that light alone will not cause full ovarian development in this species.

The effect of light intensity during uniformly short days was investigated, and it was found that 270 foot-candles was no more effective than 25 foot-candles in causing testicular development after an exposure of twenty-five days to ten hours of light per day during winter.

A quantitative evaluation of the effects of different lengths of day was obtained by exposing male and

female sparrows to day-lengths of ten, twelve, fourteen, sixteen and twenty-four hours at a uniform light intensity. In the male, gonadal development increased with day-length. During an exposure of eighteen days the greatest relative increase in testicular response occurred between day-lengths of twelve and fourteen hours; days longer than fourteen hours produced little further development. During an exposure of forty-six days, however, it was shown that males could be brought to full breeding condition in winter by a day-length of only ten hours. In the female, as in the male, gonadal growth increased with increased day-length; but during a thirty-day exposure the greatest relative increase in ovarian development occurred between day-lengths of sixteen and twenty-four hours, and no ovarian growth occurred on day-lengths of eight or ten hours. During an experiment lasting approximately 3½ months, females kept on a uniform day-length of eight hours showed no gonadal growth.

The difference in the degree of reproductive response of males and females to light is probably explained by the fact that the testis responds to lower concentrations of gonadotropic hormones than does the ovary, rather than by a difference in the sensitivity of the male and female pituitaries to light.

Light intensity in Nature falls low enough to modify the photoperiodic response of the sparrow only near sunrise and sunset. The presence or absence of clouds may cause a difference between the photoperiodically effective length of consecutive days, which is as great as the seasonal change in day-length during the month of January.

In the English sparrow an internal rhythm is important in determining the breeding season. Increased day-length in the spring reinforces this internal rhythm and ensures that all males reach reproductive competence in the spring rather than at some other season. The time of onset of breeding determines the onset of the reproductive refractory period. Consequently, the spring breeding season causes this refractory period to occur during the autumn and early winter. The end of the refractory period in turn determines the time when the sparrow can again respond to day-length. As a result, in this species the season of reproduction is controlled by the interrelationship between the seasonal change in day-length and the internal reproductive rhythm.

T. H. HAWKINS

PRISON CAMP GEOLOGY

THE fascinating memoir, referred to below*, is worthy of notice, not only because it is a major contribution to 'front' petrology and deep-seated tectonics, but also because of the extraordinary conditions under which the research it records was carried out. The University of Edelbach was founded by French prisoners-of-war in Oflag XVII *A* (1940–45). Not content with lectures alone, the geologists made a thorough investigation of the area—only 400 metres square—enclosed within the barbed wire. No stone was left unturned, and trenches and secret tunnels provided many critical exposures. A microscope was constructed in the camp and equipped with polarizers improvised from piled cover glasses. Thin sections were mounted with a mixture of violin wax and edible fat. Only the determination of certain untwinned feldspars remained to be completed on the return to France.

The greater part of the memoir is devoted to the crystalline rocks of the Waldviertel complex and the syntectonic granitization phenomena displayed by them. The country rocks are tectonites with pronounced linear structures. They include biotite- and graphite-schists, and types ranging from plagioclasite to amphibolite, all originally poor in quartz. Some excellent examples of micro-tectonic analysis are given, and it is shown that in spite of the intense deformation undergone, lattice discontinuities have been largely healed by granoblastic 're-cooking'. Movement and recrystallization were probably simultaneous, rather than alternating, phenomena. Granitization and what was formerly styled 'injection', due to geochemical migrations, consisted of quartzification, followed by development of perthitic orthoclase at the expense of both the original rock material and the newly crystallized quartz. The evidence suggests that while diffusion through the lattices took place locally, the main transport was by way of intergranular boundaries. Complementary to the addition of silicon and potassium, the cafemic elements calcium, iron and magnesium were expelled from the granitization zone to form basic fronts in the surrounding rocks. The amphibolitic aureoles of the Moldanubian 'orthogneiss' are believed to be large-scale results of the same process.

It is shown that quartz and orthoclase were remarkably plastic during the physico-chemical conditions that attended their formation, and that, in consequence, the granite formed by the transformation of pre-existing rocks could readily become intrusive. It follows that to prove a granite intrusive does not prove that it has ever been in a liquid condition.

The memoir is full of important observations and stimulating suggestions, and should be read by all workers in the field of plutonic geology.

* Métamorphisme, silicifications et pédogénèse en Bohême Méridionale. By F. Ellenberger in collaboration with R. Dézavelle, M. Fischer, A. Guilleux, V. Host, A. Moyse and P. Pérault. Pp. 169. (Besançon: Annales Scientifiques de Franche-Comté, 1948.)

FORTHCOMING EVENTS

*(Meetings marked with an asterisk * are open to the public)*

Tuesday, June 21

CHADWICK PUBLIC LECTURE (at the Royal Society of Tropical Medicine and Hygiene, 26 Portland Place, London, W.1), at 2.30 p.m.—Dr. Sibyl Horner: "Evolution of Industrial Work for Women and Young Persons and its Effect on the National Health".*

ROYAL ANTHROPOLOGICAL INSTITUTE (in the Anatomy Theatre, University College, Gower Street, London, W.C.1), at 5 p.m.—Mr. G. R. G. Worcester: "The Boat in Anthropology".

Tuesday, June 21—Wednesday, July 20

ROYAL ANTHROPOLOGICAL INSTITUTE (at 21 Bedford Square, London, W.C.1), at 10 a.m. each day.—Exhibition of "Traditional Art of the British Colonies".*

Wednesday, June 22

BRITISH PSYCHOLOGICAL SOCIETY, MEDICAL SECTION (at the Medical Society of London, 11 Chandos Street, Cavendish Square, London, W.1), at 8 p.m.—Symposium on "Aggression in Nature and Society". (All members of the Society are invited.)

Thursday, June 23

MINERALOGICAL SOCIETY (at the Geological Society, Burlington House, Piccadilly, London, W.1), at 5 p.m.—Scientific Papers.

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

AGRICULTURAL ECONOMISTS and ASSISTANT AGRICULTURAL ECONOMISTS (Grade B)—The Registrar, The University, Manchester 13 (July 4).

ANALYTICAL CHEMIST at the Board's Research Establishment, Stoke Orchard, near Cheltenham, for research in analytical methods—The National Coal Board, Grosvenor Place, London, S.W.1, endorsed TT/142 (July 5).

PHYSIOLOGIST to take part in work on the environmental physiology of farm animals—The Secretary, Hannah Dairy Research Institute, Kirkhill, Ayr (June 24).

ASSISTANT PHYSICIST in the Department of Physics applied to Medicine—The Secretary, Middlesex Hospital Medical School, London, W.1 (June 27).

SENIOR TECHNICIAN IN THE BACTERIOLOGICAL LABORATORY at Galway Central Hospital—The Secretary, Galway County Council, County Buildings, Galway (June 30).

ASSISTANTS (2) IN MATHEMATICS—The Secretary, The University, Aberdeen (June 30).

SCIENTIFIC OFFICER IN THE FIELD BOTANY DIVISION of the Ministry of Agriculture, Northern Ireland—The Secretary, Civil Service Commission, Stormont, Belfast (June 30).

ANALYST (Mineral Nutrition of Plants and Soils)—The Secretary, Long Ashton Research Station, Long Ashton, Bristol (June 30).

LECTURER IN SOCIAL ANTHROPOLOGY—The Registrar, King's College, Newcastle-upon-Tyne (July 1).

LECTURER IN MATHEMATICS, a LECTURER IN PHYSICS, and a LECTURER IN CHEMISTRY—The Registrar, Loughborough College, Loughborough (July 2).

ELECTRONICS TECHNICIAN to be responsible for the development and maintenance of equipment for the detection and measurement of ionizing radiations—The Director, Radiobiological Research Unit, Atomic Energy Research Establishment, Harwell, Didcot, Berks (July 2).

READERSHIP IN ANATOMY at Guy's Hospital Medical School—The Academic Registrar, University of London, Senate House, London, W.C.1 (July 7).

CHEMISTS for applied research work and adaptation of laboratory practice to pilot plant production, at a Ministry of Supply Research and Development Establishment in South England—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting F.393/49A (July 9).

EXPERIMENTAL OFFICER with experience in fuel technology and boiler water control, and an ASSISTANT EXPERIMENTAL OFFICER for chemical analysis of ferrous and non-ferrous metals and alloys, Ministry of Supply—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting F.380/49A (July 9).

LECTURER IN THE DEPARTMENT OF GEOLOGY—The Registrar, University College, Cathays Park, Cardiff (July 9).

ENGINEER (II or III) in the Ministry of Supply Inspectorate of Electrical and Mechanical Equipment—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting No. C. 725/48A (July 9).

CHEMISTS (Experimental Officer grade) in Ministry of Supply Research and Development Establishments mainly in South England—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting F.411/49A (July 9).

PHYSICISTS and MATHEMATICIANS (Experimental Officer grade) in Ministry of Supply Research and Development Establishments in Southern England and South Midlands, for duties mainly concerned with aerodynamics, including wind tunnel work and flight testing, etc.—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting A.898/48A (July 12).

PROFESSOR OF ORGANIC CHEMISTRY—The Registrar, Indian Association for the Cultivation of Science, Calcutta 12, India (July 15).

SENIOR RESEARCH RADIO-PHYSICIST to conduct research on angles of arrival of short-wave signals from overseas, and a LECTURER IN MECHANICAL ENGINEERING, at Auckland University College, New Zealand—The Secretary, Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1 (July 15).

TUTORIAL STUDENTSHIPS IN BOTANY, GEOLOGY, MATHEMATICS, PHYSICS, PHYSIOLOGY, ZOOLOGY and HISTORY—The Registrar, King's College, Strand, London, W.C.2 (July 16).

SENIOR LECTURER IN APPLIED NUTRITION, and a SENIOR LECTURER IN EPIDEMIOLOGY AND BIOSTATISTICS, in the Department of Social Medicine and Hygiene, and a LECTURER or ASSISTANT LECTURER IN THE DEPARTMENT OF PHARMACY, King Edward VII College of Medicine, Singapore—The Secretary, Inter-University Council, 8 Park Street, London, W.1 (July 16).

ASSISTANT LECTURER IN GEOLOGY—The Registrar, King's College, Strand, London, W.C.2 (July 16).

PROFESSOR OF PHYSICS, and a PROFESSOR OF ZOOLOGY—The Registrar, The University, Allahabad, India (July 31).

BIOCHEMIST as research assistant in the Department of Chemical Pathology—The Dean, St. Thomas's Hospital Medical School, London, S.E.1 (August 2).

PRINCIPAL—The Clerk to the Governors, Heriot-Watt College, Edinburgh (August 22).

SENIOR LECTURER IN GENERAL SCIENCE AND SCIENTIFIC METHOD in the University of Melbourne—The Secretary, Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1 (August 31).

TECHNICIAN IN THE DEPARTMENT OF BACTERIOLOGY—The Superintendent, Royal Infirmary, Sheffield 6.

LECTURER (Grade II) IN ORGANIC CHEMISTRY, an ASSISTANT LECTURER IN PHYSICAL CHEMISTRY, a DEMONSTRATOR IN PHYSICAL CHEMISTRY, and a DEMONSTRATOR IN ORGANIC CHEMISTRY—The Registrar, The University, Nottingham.

SENIOR LECTURER IN BOTANY—The Principal, Sir John Cass Technical Institute, Jewry Street, London, E.C.3.

EXPERIMENTAL OFFICER to assist in studies of Bee Behaviour—The Secretary, Rothamsted Experimental Station, Harpenden, Herts.

PATHOLOGIST, and a LABORATORY TECHNICIAN, in a research team to conduct investigations on loiasis in the Kumba area of the British Cameroons—The Under-Secretary of State (Research Department), Colonial Office, Sanctuary Buildings, Great Smith Street, London, S.W.1.

ASSISTANT PHYSICIST in the Physics Department of the Sheffield National Centre for Radiotherapy—The Secretary, Sheffield Regional Hospital Board, Broom Cross, Tree Root Walk, Sheffield 10.

ANALYST (with honours degree)—The Director of Research, Research Association of British Flour-Millers, Cereals Research Station, Old London Road, St. Albans.

LECTURER IN THE DEPARTMENT OF PHYSICS AND ELECTRICAL ENGINEERING, a LECTURER IN THE DEPARTMENT OF CHEMISTRY AND METALLURGY, and LECTURERS (2) IN THE DEPARTMENT OF MATHEMATICS—The Director of Studies, Royal Naval College, Greenwich, London, S.E.10.

ASSISTANT to participate in the research programme on the genetics of mice—The Secretary, Institute of Animal Genetics, West Mains Road, Edinburgh.

BIOCHEMICAL TECHNICIAN—The Dean, St. Thomas's Hospital Medical School, London, S.E.1.

PROFESSOR OF BACTERIOLOGY—The Dean of Medicine, University of Alberta, Edmonton, Alberta, Canada.

SCIENTIFIC ASSISTANT IN THE INFORMATION DEPARTMENT—The Information Officer, British Scientific Instrument Research Association, 17 Princes Gate, London, S.W.7.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Department of Scientific and Industrial Research: Forest Products Research. Bulletin No. 22: Identification of Softwoods by their Microscopic Structure. By Dr. E. W. J. Phillips. Pp. iii+56+3 plates. (London: H.M. Stationery Office, 1948.) 1*s.* 3*d.* net. [13

Commonwealth Bureau of Animal Nutrition. Technical Communication No. 16: Diet in relation to Reproduction and the Viability of the Young, Part 1: Rats and other Laboratory Animals. By F. C. Russell. Pp. 99. (Aberystwyth: Commonwealth Agricultural Bureaux, 1948.) 6*s.* [13

British Journal of Surgery. War Surgery Supplement No. 2: Wounds of the Extremities. Edited by H. J. Seddon. Pp. 261-374+x. (Bristol: John Wright and Sons, Ltd., 1949.) 12*s.* 6*d.* net. [13

Department of Industry and Commerce: Geological Survey of Ireland. Emergency Period Pamphlet No. 3: Sources of Industrial Silica in Ireland. By D. W. Bishopp and J. A. G. McCluskey. Pp. 48+2 plates. (Dublin: Stationery Office, 1948.) 1*s.* 6*d.* [13

British Standard 771: 1948. Synthetic Resin (Phenolic) Moulding Materials. Revised edition. Pp. 42. 5*s.* net. British Standard 1943: 1948. Polystyrene Moulding Materials. Pp. 14. 2*s.* net. (London: British Standards Institution, 1948.) [83

Air Ministry: Meteorological Office. Meteorological Reports No. 1: Aviation Meteorology of South America. (M.O. 496a.) Pp. 50. (London: H.M. Stationery Office, 1948.) 5*s.* net. [83

Medical Research Council. Special Report Series No. 264: Vitamin A Requirements of Human Adults; an Experimental Study of Vitamin A Deprivation in Man. Compiled by E. M. Hume and Dr. H. A. Krebs. Pp. 145. (London: H.M. Stationery Office, 1949.) 3*s.* net. [83

Dust in Industry. Papers read at the Conference at Leeds, 28th-30th September, 1948, with the Discussions which followed. Pp. v+175+18 plates. (London: Society of Chemical Industry, 1949.) 42*s.* [143

Commonwealth Bureau of Horticulture and Plantation Crops. Technical Communication No. 20: Growth Substances and their Practical Importance in Horticulture. By Dr. H. L. Pearse. Pp. 233. (Aberystwyth: Commonwealth Agricultural Bureaux, 1948.) 12*s.* 6*d.* [153

Air Ministry: Meteorological Office. Instructions for the Preparation of Weather Maps; with Tables of the Specifications and Symbols. (M.O. 515.) Pp. ii+44. (London: H.M. Stationery Office, 1949.) 9*d.* net. [153

British Standard 1524: 1949. Cellulose Acetate Moulding Materials. Pp. 26. (London: British Standards Institution, 1949.) 3*s.* net. [153

Commonwealth Bureau of Soil Science. Technical Communication No. 45: The Practice of Soil Conservation in the British Colonial Empire. By Sir Harold A. Tempany. Pp. 106. (Aberystwyth: Commonwealth Agricultural Bureaux, 1949.) 10*s.* [153

Proceedings of the International Congress on Population and World Resources in relation to the Family, August 1948, Cheltenham. (Published for the Family Planning Association of Great Britain.) Pp. xviii+246. (London: H. K. Lewis and Co., Ltd., 1949.) 10*s.* 6*d.* net. [153

Other Countries

Analytical Chemistry and Chemical Analysis, 1948. Proceedings of the International Congress on Analytical Chemistry, Utrecht 1948; Opening Session, Sections 1, 2, 4, 5, Final Session. Pp. x+417-854. (New York and Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948.) 25*s.* [13

Tellus: a Quarterly Journal of Geophysics. Vol. 1, No. 1, February 1949. Pp. 64. (Stockholm: Department of Meteorology, The University, 1949.) 2 U.S. dollars; 6 U.S. dollars a year. [83

Baltic Geodetic Commission. Special Publication No. 10: Adjustment of the Baltic Ring. By V. K. Ölander. Pp. 70. (Helsinki: Baltic Geodetic Commission, 1949.) [83

Abhandlungen der Deutschen Akademie der Wissenschaften zu Berlin: Mathematisch-naturwissenschaftliche Klasse. Jahrgang 1945-46, Nr. 6: Ur- und Neuozeane. Von Hans Stille. Pp. 68+2 plates. 7 D. marks. Jahrgang 1947, Nr. 6: Die Ermittlung des Erregungsablaufs in ungeschädigten und geschädigten Herzen durch Analyse des Elektrokardiogramms und des Vektordiagramms. Von Lothar Wendt. Pp. 84. 12.50 D. marks. (Berlin: Akademie-Verlag, 1948-1949.) [83